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RADIANT HEAT TRANSMITTED BY INCANDESCENT SPHERICAL BODIES.

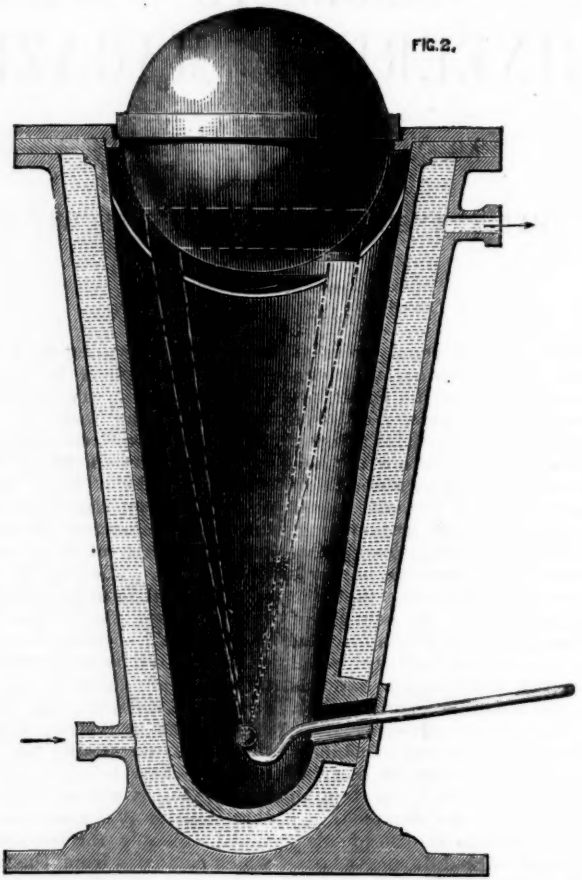
By Captain JOHN ERICSSON.

From "Engineering."

The question whether equal areas at different points of the solar surface transmit equal energy towards the earth has not been satisfactorily answered. We have seen in the previous articles relating to radiant heat that the author of "Méchanique Céleste," finding by observation that equal areas do not transmit equal energies (the central regions transmitting, in opposition to his reasoning, much greater intensity than those near the border), explains the matter by showing that the solar atmosphere retards the passage of the rays, causing a great diminution of the energy of the radiant heat projected towards the earth. It but seldom happens that questions of a cosmical nature admit of being decided by actual experiment, the present being one of the rare instances in which practical tests may be resorted to. Evidently if the great diminution of energy towards the border demonstrated in the work adverted to is caused solely by the retardation offered during the passage of the rays through the atmosphere surrounding the photosphere, the receding surface of an incandescent spherical body of any size whatever, *not* surrounded by a retarding medium, will transmit its radiant heat undiminished. The illustration on the next page shows a mechanical device by means of which it has been clearly demonstrated that the absence of a retarding medium round an incandescent sphere does not affect the diminution of energy resulting from the obliquity of the heat rays projected by the receding surface. Fig. 2 represents a vertical section of a conical vessel surrounded by a water jacket, and in other respects similarly constructed to the one delineated in the preceding article on radiant heat emitted by inclined discs. The top flange, however, of the vessel here presented is provided with a groove, the bottom of which supports a solid sphere of cast iron, in the manner shown by the illustration. Below the sphere are inserted two semispherical screens of different diameter, an annular opening being thereby formed between the same. Supposing the sphere to be heated before being placed in the position shown, it will be perceived that the thermometer at the bottom of the conical vessel will only receive the radiant heat which emanates from the zone contained within the dotted horizontal lines shown in the drawing. The heat rays from this zone, converging in the centre of the bulb, are indicated by dotted radial lines. It is evident that by changing the dimensions of the screens, zones covering *equal areas*, but occupying *different* positions, may be made to radiate towards the thermometer, and that, by this means, the

radiant power of any portion of the sphere may be accurately ascertained. We are accordingly enabled to test the correctness of the assertion, that "but for the intervention of the sun's atmosphere, the receding solar surface would, owing to the increased number of rays within a given section, produce increased intensity. It may be urged against our device that atmospheric air intervenes between the incandescent sphere and the recording thermometer, but a moment's consideration will show that the consequent retardation is practically inappreciable. The retardation of the sun's rays in passing through the depth of 28,800 ft. of atmospheric air of maximum density, on the ecliptic, it has been demonstrated in pre-

FIG. 2.



vious articles, amounts to 0.207, while solar intensity at the boundary of the atmosphere is somewhat under 85 deg.; hence the loss of radiant heat will scarcely reach 17.5 deg. Fahr., notwithstanding the great depth of atmospheric air penetrated. The radiant heat of our experimental apparatus being transmitted through a depth of less than 2 ft., we may, without material error, assume that no retarding medium surrounds the experimental incandescent sphere. The principal features of our apparatus having thus been explained, and the method of solving the problem under consideration pointed out, we may now proceed to consider the result

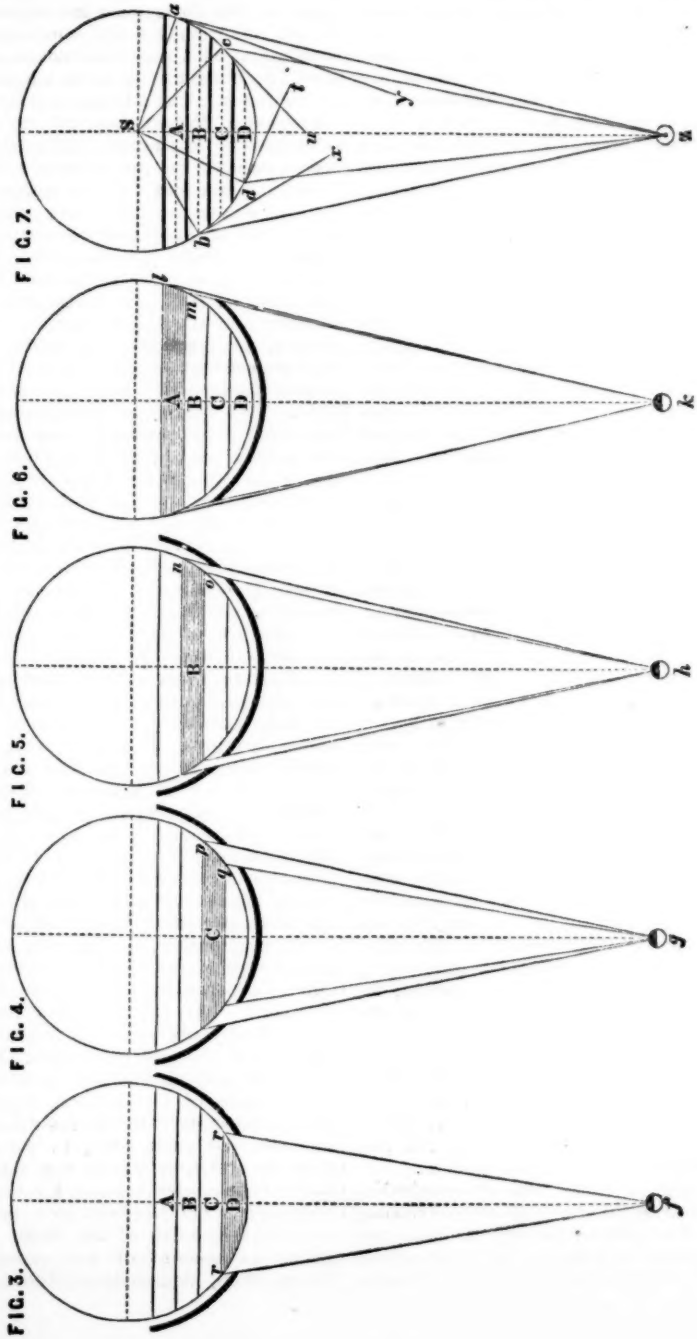


FIG. 7.
a
c
g
A
B
C
D
b
d
z
x
y
u
FIG. 6.
k
m
A
B
C
D
FIG. 5.
n
o
B
FIG. 4.
p
q
C
FIG. 3.
r
A
B
C
D

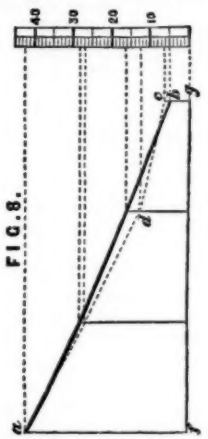
of the experiments which have been instituted. In order to facilitate comparison, the lower part of the sphere visible from the centre of the bulb of the recording thermometer (see Fig. 6), has been divided into four zones, A, B, C, and D, containing equal areas. It will be evident on inspecting the arrangement represented in Fig. 2, that no part of the surface of the sphere excepting that contained within the dotted parallel lines, is capable of radiating towards the thermometer, all the rest being shut out by the semispherical screens. Obviously the latter can be so proportioned that the radiant heat from any part of the lower half of the sphere may be projected towards the bulb. Figs. 3, 4, 5, and 6 show the arrangement of screens adopted in our experiments, by means of which the radiant power of each of the zones has been ascertained. The dimensions of the several screens have been determined by drawing radial lines from the centre of the bulb of the thermometer to the points where the termination of the zones intersect the circumference of the sphere. The subject will be most readily understood by refering to Fig. 4, which exhibits zone C. The screens being made to terminate in the radial lines, p, g, and q, g, it will be seen that an annular opening, p q, is formed, permitting all heat rays to pass which are projected from the zone C, in the direction of the bulb of the thermometer. A similar arrangement permits the radiant heat from zone B, in Fig. 5, to act on the thermometer. Referring to Fig 3, it will be found that only one screen, perforated in the centre, is required to shut out the radiant heat from the three upper zones, C, B, and A; while in Fig. 6 the radiation from the three lower zones, D, C, and B, is shut out by a single central semispherical screen, the circumference of which is defined by the radial lines, m, k. It is proper to observe that, although the several screens are represented by single lines, in the diagram, they are in reality composed of double plates, a fire-proof non-conducting substance being inserted between the two, the object of which is self-evident.

Bearing in mind the demonstration contained in the previous article relating to the diminution of energy of heat rays projected at an acute angle to the radiant surface, it will be perceived, on mere inspection, that the upper zones represented in our diagram, though containing an equal area with the lower zones, cannot posssibly transmit the same temperature as the latter. The advocates of the views expressed in "Méchanique Céleste" will be surprised to learn that, notwitstanding the absence of an intervening retarding medium, so great is the difference of energy communicated, that while the zone D, of the experimental incandescent sphere, transmits a temperature of 42.5 deg. to the thermometer, the zone A transmits only 4.7 deg. The latter zone being further from the thermometer than the former, a correction is, however, necessary on account of the increased dispersion of the heat rays before reaching the bulb. This correction being made, the true ratio of temperature transmitted by the zones D and A, will be 42.50 deg.: 6.19 deg. Consequently, the heat rays projected from the lower zone of the incandescent sphere towards the bulb of the thermometer, transmit nearly seven times higher temperature than the rays from the upper zone. The amount of radiant surface being alike in each zone, while the temperature of the sphere is uniform throughout, it will be admitted that our practical test has clearly demonstrated the feebleness of the rays of heat projected from the border of an incandescent sphere towards a given point. It is hardly necessary to add that each zone has called for a separate experiment, rendering re-heating of the sphere indispensable for each. The same expedient has, therefore, been resorted to in order to insure an equal degree of temperature during each experiment, as in the case of the incandescent inclined disc, discussed in the previous article. Of course, it has been found impracticable to impart an equal temperature to the sphere for each operation; but this difficulty has been satisfactorily overcome by establishing a mean, as in the case referred to. Besides, the result may be checked by computing the degree of temperature capable of being transmitted to the recording thermometer by each zone, in accordance with the relation which the intensities bear to the angles formed by the radiating surface and the heat rays projected towards the centre of the bulb. Before giving an account of our experiments, let us, then, demonstrate, theoretically,

what temperature each zone ought to communicate to the thermometer, in conformity with the ascertained fact, that the intensity of the radiant heat transmitted by an incandescent disc, is directly proportional to the sines of the angles formed by the projected heat rays and the radiating surface. In order to simplify the demonstration, the several zones have been divided into halves, by a dotted line, see Fig. 7; radial lines being drawn to the thermometer at Z, from the points of intersection of the dotted lines and the circumference of the sphere. Tangential lines, *d t*, *c u*, *b x*, and *a y*, have also been drawn from the points of intersection referred to. It will be evident on considering the properties of spherical zones, that the radial lines, *d* Z, *c* Z, *b* Z, and *a* Z, represent the mean direction of the heat rays projected by each zone respectively, towards Z. Hence the sines of the angles, *t d* Z, *u c* Z, *x b* Z, and *y a* Z, will determine the amount of radiant heat transmitted towards Z, by each of the zones, D, C, B, and A. Calculation shows that if the sine of the angle *t d* Z, be represented by unity, the sines of the other angles, in the order presented, will be 0.671, 0.384, and 0.121, while the experiments which have been made show that the zone D transmits a temperature of 42.50 deg. to the recording thermometer. Consequently, the zones, C, B, and A, ought to transmit respectively 28.50 deg., 16.31 deg., and 5.16 deg. to the thermometer at Z. The accompanying Table shows to what extent the

1. Zone.	2. Mean angle of projection.	3. Comparative sine.	4. Observed temperature.	5. Corrected temperature.	6. Computed temperature.
	deg. min.		deg.	deg.	deg.
D....	58 0	1.000	42.5	42 50	42.50
C....	34 40	0.671	24 2	27.49	28 50
B....	19 0	0.384	10.1	12 82	16 31
A....	5 55	0.121	4.7	6 19	5.16

actual temperatures transmitted by the incandescent sphere differ from the stated computed temperatures. It should be observed that no direct comparison can be based upon the temperatures entered in the fourth column, since the heat rays projected by the several zones are subjected to different degrees of dispersion, owing to the unequal distance from the thermometer. Due allowance being made for the dispersion of the rays, in conformity with the elements furnished in Fig. 7, the consequent augmentation of temperature has been added, and the corrected values entered in the fifth column of the Table. The computed temperatures will be found in the sixth column. It will be supposed at first sight that the figures entered in the Table indicate a serious discrepancy between the observed and the computed temperature. That such is not the case will be found on referring to Fig. 8, in which the ordinates



of the regular curve, *a b*, represent the computed temperatures, while the ordinates of the irregular curve, *a d c*, represent the observed temperatures. Obviously, the computed and the observed energies transmitted by the radiation of the incandescent sphere, is truly represented by the superficies contained between the base, *f g*, and the curves, *a b* and *a d c*, respectively, the ratio being 1.000:0.945. Considering the insignificance of this discrepancy, in connection with the difficulty of bringing the heated sphere to an equal degree of incandescence during each experiment, it will be admitted that the instituted test has proved conclusive, and that the inaccuracy of the theory promulgated in "Méchanique Céleste" regarding the radiant energy transmitted by the sun, has been fully demonstrated.

ON THE TURBINE PROPELLER.

By ANDREW MURRAY, Esq., C.B., etc.

From "The Journal of the Royal United Service Institution."

It is hoped that the importance of the subject, and the special interest it has for naval men, will be considered such as to form a sufficient excuse for bringing the system of hydraulic propulsion again before this Institution, though little has been done in respect to it since it was last discussed here. At that time the Waterwitch, built for the Admiralty on the recommendation of the late Controller, at the instigation of Vice-Admiral George Elliot, propelled on the principle of recoil from an effluent stream of water driven out by a centrifugul pump revolving horizontally in the vessel, had just been completed, and tried, and her performances were given in full.

In the great change from the paddle-wheel to the screw, the Admiralty led the way with the Rattler, and the block ships (mainly at the instigation of the Right Honorable H. T. L. Corry), appreciating its special applicability to men-of-war in comparison with the paddle-wheel, several years before it was taken up by the mercantile service; and, as they have now again led the way with the Waterwitch, in consequence of a claim having been put forward for superiority on the part of the turbine propeller over the screw, it is to be hoped that they will proceed with equal boldness in testing it to the full extent, and not allow the expense to which they have already gone to remain fruitless. It has been admitted that the results were more favorable than had been anticipated by the professional Officers of the Controller's Department in London, when the experiments were first sanctioned (though quite in accordance with those promised in the Report that led to this sanction being given), and this being so, there appears to be no just grounds for not continuing and extending the trials.

The principle on which the turbine propeller acts may be briefly explained by stating that as a gun recoils on the explosion taking place, or as the progress of a rocket is kept up by the recoil arising from the efflux of the gases generated by the ignition of the composition with which the rocket is filled, so the progress of the vessel is kept up by the recoil arising from the efflux of a stream of water in a sternward direction, kept up by the action of a centrifugal pump or turbine, driven by a steam-engine, drawing water from the sea and discharging it sternwards in a continuous stream through a bent pipe or nozzle at a high velocity. The principle may also be illustrated by supposing a cylinder closed at the ends and filled with an elastic gas or vapor in a highly compressed state, and therefore at a great pressure, in which case the pressure would be equal in all directions on the surface of the cylinder, and there would be no action beyond a bursting tendency; but if one end were to be suddenly removed the pressure on the opposite end would be no longer balanced, and the pressure would produce motion by its action, if it should be sufficient in amount to overcome the weight or inertia, or resistance of the cylinder or vessel.

The object of the present paper is to endeavor to awaken interest in the turbine again, and to enlist additional advocates amongst naval men for further experiments. The success of the Waterwitch, as a first attempt in the Government service, was certainly as great as was that of the Rattler, and this success ought to outweigh the misgivings and counterbalance the adverse views expressed by men of science, and which it is believed have tended much to retard the progress of the system up to this time. When the determination of the Admiralty, in 1864, to sanction the construction of the Waterwitch, under the patent of Mr. Ruthven, became known, it gave occasion to several mathematicians to investigate the question afresh, and bring to bear upon it some new views in fluid dynamics. In more than one instance the utter failure of the Waterwitch was predicted in the most confident manner from these investigations, but the degree of success she attained may be taken as evidence of the difficulty of any investigation of this kind, and that too much reliance had been placed by those writers on the formulæ adopted by them. In a

recent discussion at the Institution of Civil Engineers, on the best form of the Archimedean screw for raising water, and on centrifugal pumps, the Astronomer Royal stated that the motions of fluid particles amongst themselves were, in his opinion, too complicated to be reduced to any definite formulæ sufficiently simple to allow practical deductions to be drawn from them. With such an authority to quote, further reasoning as regards those calculations that start from theoretical grounds only, may be dispensed with; but there are other mathematical investigations that claim to be founded on practical facts, and it may be well briefly to consider these. They must, of necessity, involve a calculation of the amount of loss resulting from the friction of the particles of water amongst themselves and against the sides of the pump, especially if a centrifugal pump be used, and against the sides of the pipes or passages to and from the pump. In centrifugal pumps of 40 or 50 horse-power, used for raising water, which, though considered of a large size for such a purpose, might almost be looked upon as models in comparison with those which must be used in the propulsion of ships of any size, the results realized have been found by actual test to vary from about 50 to 80 per cent. of the full power expended, the difference arising from mere differences in the form of the blades and other details of construction. With this fact patent to us, how can any calculation of the speed to be expected from a ship be depended on, when that speed can be materially affected by the efficiency, or otherwise, of such minute points? In attempting a comparison, however, between the paddle-wheel, or screw, and the turbine, to lay before this Institution, on general grounds, without entering into any minute points of science or engineering, it will be necessary to recall to mind some of the facts, and the deductions drawn from them respecting the former. In the common paddle-wheel a considerable portion of power is lost from the obliquity of the action of the paddle-board or float in its revolution as it enters and leaves the water, the reaction of the water not being at all points in the line of direction in which the effect is desired to be produced upon the vessel, and other portions of power are lost from setting a portion of water in motion in a sternward direction, and from the float lifting or raising, and throwing backwards a portion of water as it rises.

In the use of the screw, likewise, a portion of water is set in motion in a sternward direction, and power is lost in proportion in obtaining the reaction or the push required for the propulsion of the ship. In a very able paper on the action of the screw propeller, by Mr. Froude, he illustrates this by supposing a separate ideal current to be in existence where the screw works, flowing in the direction of the ship's motion, without its acting on the ship, and that the propulsion is obtained by each blade of the screw in its rotation meeting so much of this current and acting upon it with exactly that force that is sufficient to stop it, and thus, by raction, obtaining back from it the exact force exerted. He considers that this would be a perfect system of screw propulsion, no power being wasted in giving motion to any particles of water, but the whole made useful in propelling the ship, except that absorbed in overcoming the friction of the surface of the screw.

That the amount of power so lost by the paddle-wheel is great is admitted by all writers upon it, and it is also admitted by the writer on the screw, before quoted, that a great deal of force is exerted by it to no purpose.*

It is difficult, however, to determine the amount of this loss separate from the power lost by the engine itself in the friction and the working of its own various parts, and this is not attempted in that paper. Aristides Mornay, in the last edition of "Tredgold on the Steam Engine," estimated it for the ordinary paddle-wheel at above 40 per cent. for the oblique action of the floats on the water, without taking into account the loss from putting the water in motion. The loss of power from the screw has been variously estimated, but from the early experiments with the dynamometer in the Rattler, and from other data, it may be inferred that the loss does not vary much from that of the paddle-wheel; but in both these cases the losses named are little more than mere estimates.

* Froude, "Transactions of Naval Architects' Institution," 1867, page 77.

The amount of loss arising from the action of the steam-engine and the propeller combined, compared with the gross power developed in the cylinders of the engine, as shown by the indicator diagrams, is more within our reach. If this total loss can be ascertained, when different propellers are used, good grounds are obtained for judging of their relative efficiency, because it may very well be inferred, or rather proved, that in all well made steam-engines of the same horse-power, and of similar arrangements in their parts, the amount lost will not vary much, as all the parts involving the great proportion of the loss by friction must of necessity be nearly alike. If the average total pressure on the surface of the pistons of a pair of direct-acting paddle-wheel engines be obtained from the indicator diagrams, and the total number of pounds, or tons, acting on the end of the crank be found from this, the equivalent weight acting at the centre of pressure of the paddle-board that would balance that amount, can be found as a mere question of leverage.

The actual weight, or force, exerted at the centre of pressure of the paddle-float, instead of the weight so found to balance the total pressure on the pistons, has been made the subject of direct experiment, by attaching a vessel by a hawser to a dynamometer in the form of a bent steel yard fixed on shore, and setting the engines to work, and though the motion of a vessel through the water alters the circumstances under which the paddle-wheel or the screw acts, yet the result is interesting and useful, not only as showing the amount of power lost, but as giving a means of comparison of the effect from different forms of paddle-wheels, or screws, or propellers, of any kind. If the same propellers be fitted to similar ships with different kinds of engines, it would show the difference in the effective powers of their engines. It also illustrates the little effect produced by a steamer attached to a vessel on shore to drag her off, and that the mere push of a vessel without way, or with but little way, on her, would be comparatively useless as a ram to run down an enemy, a point that seems to have been rather too much lost sight of in some of the discussions on this subject. As an example, a small paddle-wheel vessel, with engines of 150-horse power nominal, the cylinders being 48 in. diameter, and the length of stroke 3 ft. 9 in., was tried in this way. The average pressure on the pistons was found by the indicator diagrams to be 18.16 lbs. per. sq. in., this gave a total pressure of 30 tons on the two pistons acting on the crank at a leverage of 1 ft. 7½ in., half the stroke; the paddle-wheel was 13 ft. diameter to the centre of pressure of the float, and the weight at the radius of 6 ft. 6 in. to balance 30 tons at 1 ft. 7½ in. would be 4.615 tons, but instead of this the actual pull, as shown by the steel-yard on shore, was only 3.078 tons, a loss of more than one-third in direct pressure, while the loss of effect in horse-power by taking into account the speed or travel of the piston and the travel of the centre of effort of the paddle-float, was from 369.6 indicated horse-power in the cylinders to 204.4 indicated horse-power on the paddle-float, or in the ratio of 100 to 55.3. This gives a dead pressure or pull of about 2 tons only for every 100 indicated horse-power when the vessel has no way on her, and thus proves the loss incurred when using the paddle-wheel as a propeller, other vessels when tried in the same way giving similar results.

The total loss from the action of the engine and the screw combined has been estimated at about one-third of the gross indicated horse-power in the cylinders.

From the circumstance that no loss from putting water in motion in a sternward direction in the manner before referred to, occurs with the turbine, it has been argued that it must, therefore, be a more efficient propeller, and that it has no slip. The meaning intended to be conveyed by this latter expression is obvious, but it may be doubted whether it is correct. With the turbine it would seem that a loss of power must take place in imparting a forward motion, equal to the speed of the vessel, to the water which is being constantly taken up from the sea in a state of rest, and carried along with the ship, before it is thrown out at the nozzles, so that this may to some extent be equivalent to the loss incurred by the paddle-wheel or screw in putting water in motion in a sternward direction. Again, the speed of the ship is not necessarily the same as that of the water issuing from the mouth of the nozzle, and if this difference

were called the slip of the turbine, it might be more in accordance with the term as applied in the other case, though it does not prove that any loss of power arises from any such difference.

The question, then, of the relative efficiency of these different propellers seems to narrow itself very much into the question of the efficiency of a centrifugal pump or the percentage of effect to be obtained from it. With this, as before stated, the loss has been tested by actual experiment, and has varied from 20 up to 50 per cent., so that if the full value of the reaction be obtained, there seem to be grounds for believing that the turbine will prove as efficient as the others. It has been questioned, however, whether the effect of a centrifugal pump in raising water affords just grounds for estimating its efficiency as a propeller. In the former the object desired is the raising or delivering the greatest quantity of water possible, while with the propeller the objects are area and pressure; but as the quantity delivered will be in accordance with the area of the discharging orifice and the velocity of the effluent water, and as velocity and height of discharge can only be got by pressure, it would seem that the objects desired are so far identical, and that the pump which showed the greatest efficiency in the one case would also show the greatest efficiency in the other.

To revert to the facts of the case between the Waterwitch and the sister vessels the Viper and the Vixen, the final results which were obtained in August, 1867, after the first preliminary experimental trials, were a speed of 9.237 knots, with 777 indicated horse-power, for the Waterwitch, against 9.475 knots, with 652 indicated horse-power, for the Viper, and 9.060 knots, with 658 indicated horse-power, for the Vixen. (Full details of the vessels and trials are given in an appendix.) Such a performance for so early an attempt may certainly be claimed as a great triumph for a system so completely different from the other modes of propulsion in use. It places the turbine almost on a par with the screw, and it is to be regretted that the patentees were not satisfied with this, but allowed their partiality and their enthusiasm in favor of their invention to carry them away so far that they claimed 20 to 30 per cent. advantage for the turbine, and affirmed that the official returns understated the horse-power exerted in the Viper and the Vixen, and also that the engines of the Waterwitch did not give out the power that was due from them and from the boiler. These claims and statements have undoubtedly tended much to retard the progress of the system, parties who were favorable to it being unable to endorse them, and inquirers, in consequence, became puzzled to know how much to believe, and hung back. It is not proposed now to answer these points in detail, but it may be generally stated that they are based on scientific considerations. It is argued by the patentees that the results and the coefficients of performance derived from the first trials of the Viper and Vixen at the Maplin Sands, are irreconcilable with those derived from the subsequent trials of the same vessels at Stokes Bay, and they therefore wish to discredit the whole results. The discrepancy referred to, however, is amply accounted for from the fact that in these vessels which are built with 2 after-bodies, 2 stern-posts, and 2 rudders, with a screw in each after-body, the screws used in the first trials at the Maplin Sands were inefficient. There is a space between the two after-bodies which may be looked upon as a covered passage way, the roof of which slopes down to meet the water and then onwards to the bottom of the ship, and the water in this space is necessarily in a very disturbed state. The two screws with which the vessels were tried at the Maplin Sands turned inwards towards this disturbed water, there being little or no previous experience on this form of a vessel's body; but new screws were made for the trials at Stokes Bay, and these were arranged to turn outwards, and the effect was so different that a discrepancy between the results derived from these two sets of trials cannot be looked upon as a matter of surprise, as if all the circumstances had been alike. The differences in the coefficients derived from the one set of trials of these vessels at Stokes Bay are not greater than are found in ordinary cases. In the Philomel, whose performances are quoted and argued upon by the patentees of the turbine as reliable, the coefficient varies from 302 to 316, a difference of 14, while in the Viper it varies from 426 to 439, a difference of 13, for the full power trials. The Vixen was tried once only. Calculations were also made

from the number of cubic feet of steam calculated to have been used in the cylinders, taking the indicator diagrams as the foundation for this calculation, and it was argued from these that the engines were not developing a proper amount of power; but the researches of Professor Tyndall, Professor Rankine, and others, have shown that the absorption of heat and the condensation of steam which are found in all engines, and wherever motion takes place, are such, that a calculation of this kind does not rest on a sound foundation.

Professor Main investigated this question in the Bee, at Portsmouth, many years ago, and naval officers do not require anything more than the mention of his name, and that of Mr. Brown, to feel satisfied that the experiment would be conducted with the greatest care, and the results be scrutinized with intelligence and industry, and he failed entirely, as stated in his work on the marine engine, in bringing the amount of steam, as shown by the indicator in the cylinders, into accordance with the amount of water known to be evaporated by the boiler. This experiment was made by him long before the researches of Professor Rankine, who has shown why they cannot be in accordance. Those calculations, therefore, afford no grounds for the assumption that less net effective power was given out by the engines of the Waterwitch to the turbine, than is given out by other engines to the propellers which they may have to drive.

The indicated horse-power shown by the "Philomel" class, in comparison with that shown by the Viper and Vixen, all being of the same nominal horse-power, has been relied on as a proof that the power exerted by the engines of the latter was understated in the official returns, but the Philomel is of later construction, and her boilers are different, and though the diameters of the cylinders and the length of stroke of the engines are the same, yet the fact that these boilers are loaded to 30 lbs. pressure, instead of 22 lbs., and that her engines made 118 and 119 strokes against 108 and 109, it is evident that the indicated horse-power may well be increased, and no argument can, therefore, be founded on this to prove that the amount of power realized by her engine was not in reality greater than that realized by those of the Viper and Vixen.

A further attempt to prove the same point was based on the relative midship sections and speeds of the Philomel and of the Viper and Vixen, that the latter vessels attained the speed they did with less power relatively than the Philomel, for the speed that she attained, and that this must be wrong, because they are fuller and carry armor, while the Philomel is unarmored, is of less beam, and is longer. The first argument referred to, as used by the patentees of the turbine, was based on the coefficient of effect derived from the midship section, which they state is received as a distinct proof of the value of a propeller, and that any departure from the law on which this coefficient is based must be an error. Upon this ground, then, the assumption of an error as regards the power exerted in the Viper and Vixen is disproved, because the coefficient for the Philomel is lower than that for these vessels, being 419 against 439 and 433 —a very material amount of difference. Why the Philomel class have so low a coefficient does not affect the question, and need not be entered on here.

To return to the fact that the results of the trials of the Waterwitch at Stokes Bay, which were officially reported from Portsmouth, and which were accepted at the Admiralty as correct, placed her performances, as before stated, nearly on a par with the screw in the sister vessels, Viper and Vixen, with which she was built to compete; it is desired by some to look upon these results now as not conclusive evidence in favor of the turbine, but she certainly would not have been built in her present form if there had been any obvious reason why the comparison between her and the Viper and Vixen would not be a satisfactory test. None of the parties interested made any objections at that time. It is greatly to be regretted that all these vessels, both turbine and screw, should have turned out unfit to be kept in commission, and used along the coast, but even with the Waterwitch, such as she is, many interesting experiments might be made, and, indeed, ought to be made, if I may be permitted to say so, before another turbine ship is attempted, as many modifications present themselves as worthy of trial.

The mode of introducing the water into the vessel has been a subject of comment and discussion, but there seem to be good reasons for believing that the introduction

of it at the bottom by slits sloping upwards into a canal, as in the Waterwitch, is to be preferred to an entrance at the bow of the vessel, or at the sides. The only objection that has been urged with much show of reason has been, that the proper supply of water might be interfered with if the vessel took the ground on a flat shore, with stiff or gravelly clay under her; but this is an occurrence of so exceptional a character, that it need scarcely influence so important a point for her general employment, and there would probably be a better chance of getting her off from such a position by laying out an anchor and hauling upon it, than by working the engines and getting so small a pull upon her as we have previously shown they would give under such circumstances. A proposal has also been lately made by the Chief Engineer of the Italian Government, who takes a favorable view of the system theoretically, to introduce the water from abaft; but it is to be feared that the speed of the vessel through the water would not allow time for the limited pressure of the water outside to force a sufficient supply into the passages, even though the pump created a vacuum for it to flow into. The present arrangement also gives facilities for sluices to admit a large supply of water from the interior of the ship, in the event of a leak, as well as for stopping off the supply from the sea when the turbine is not at work.

The form of the turbine itself, however, is the most important question. It has been already stated that the efficiency of different centrifugal pumps has been found to vary as much as 25 per cent. In the Waterwitch, the blades of the turbine are made to curve in exactly the opposite direction to those in Appold's pumps and in the most approved pumps of the present day, and though these were so made, under the patentee's express instructions, as he considered it the best form, it would be most important to have a new set of arms or blades made with the curvature, and to test the effect, more especially as this could be done without altering the outside case and the work connected with it, and would consequently be comparatively inexpensive.

If the views respecting the pump as a propeller, as previously expressed, be correct, and the pump be made thoroughly efficient, so as to bring into useful effect a large percentage of the power brought to bear upon it by the steam-engine, then the propeller must be also efficient by the law of science, that action and reaction are equal, that is, "that the total momenta impressed by any force in one direction must be looked for by equivalents, to reappear in inverted momenta in the opposite direction." These are the words of Mr. Reed, at the Naval Architects' Institution, when arguing upon a paper by Professor Rankine, who replies, "that the whole of his paper is founded on that very dynamical principle to which Mr. Reed refers, in combination with another principle, as the oscillatory character of wave motion."* This definition of reaction is quoted as being clear, and specially applicable to the turbine, and the reply is referred to as an instance of the difficulty that even scientific men find in understanding and following deep mathematical investigations of this kind made by themselves, as previously referred to in this paper.

The vertical position of the shaft of the turbine is a great drawback to the application of engines of ordinary construction, but it is by no means improbable that a pump may yet be made with the shaft running horizontally, and this would undoubtedly remove much of the hesitation of engineers to recommend the principle to their clients the shipowners for a trial.

The form and size of the discharge nozzles might also be experimented on with advantage. There seem to be grounds for believing that a very much larger orifice would give a greater propelling power, though attended with diminution of pressure. Centrifugal pumps have not been found good for high lifts, from which it may be inferred that it is difficult to obtain pressure in them, and their efficiency is best displayed in the large quantities of water which they deliver at a moderate height of the power employed. There are difficulties in the way of enlarging the nozzles of the Waterwitch to test this, but there would be none in reducing them, and trying the effect, testing the pressure and the actual velocity of the water with the different sized nozzles.

* Transactions of Naval Architects' Institution, 1867, page 99.

It is believed that this velocity has never yet been tested, and that all the calculations as to the number of tons of water discharged have been made from an assumed velocity, founded on the speed of the blades of the pump.

It does not necessarily follow that the pressure will be increased by making the orifice smaller, because if the discharge be not sufficiently free after a certain pressure has been attained, the water will remain in the pump, and revolve with the blades, or lie dormant in the space between the ends of the blades and the casing. This is found to be the case with fan-blowers, the steam-engine running faster, showing less resistance and less work done when the escape of the full quantity of air is prevented by lessening the orifice for its exit.

It has been assumed by some that the speed of the effluent current at the nozzle ought to be about 10 per cent. above the speed that previous calculation would give to the ship, but further experiment is required before this can be received in any way as definite or as a best proportion of one to the other.

The height of the orifice above the water is also a question that is open to discussion, but there do not appear to be grounds for believing that there would be any important advantage in placing it below water, and if this be the case there are certainly great practical advantages in continuing it above water, where it can be got at with facility, and by which the pump can be kept empty. When the orifice is near the surface of the water, no power of any consequence is lost in raising it, as it will of itself rise in the pump to the level of the water outside.

While the experiments that have been herein proposed would be important, the alteration that one would most wish to see carried out in the Waterwitch would be to put a new bow and a new stern to her, and raise upon her. This would leave all the most expensive part of the ship, with the machinery in it, untouched, or if the ship were merely raised upon with skeleton work, as was done with the Cerberus, to make her seaworthy, much useful information might be obtained.

The further advantages of the turbine propeller in stopping the way of a vessel rapidly, at the command, and under the hand of the officer in charge, as well as its great power over a heavy leak, its freedom from the chance of internal injury and of being fouled, and the comparative ease with which it can be protected from shot, have all been brought before the Institution in the paper formerly read, and they appear to be admitted.

The power of the turbine propeller to turn a vessel round with rapidity has disappointed its advocates to some extent on this point, but it has been proved that a vessel's steerage is very much assisted by it, and in the case of the very long ship now being introduced into the merchant service, as sailing and steaming clippers, the assistance that it will render in enabling their heads to be kept to the sea, and preventing them from falling into the trough of the sea, will be very important, and make it not only equal, but superior, to the twin screw system in this respect, as the captain can manage the whole as easily as he can the steering wheel.

For vessels of light draught, especially of large size, such as are required for the Danube, the Volga, the Ganges, or the Indus, and others, the turbine has been put forward as specially applicable, but the paddle-wheel may compete with it for speed in these, and it must rely on its own intrinsic merits, and not on the inapplicability of other systems of propulsion, for its adoption in these as in other vessels.

Its advantage in combination with sails, though previously mentioned, may perhaps be dwelt upon a little further, as this has not perhaps been done to the extent that it might have been by its able advocate Admiral Elliot, in his anxiety to show its peculiar fitness for small vessels for end on fighting with a rudder in the bow and prepared to steam either way. In speaking of this point for a man-of-war cruiser or for a merchant ship making a long voyage, it is not merely that the action will be better in a sea-way from its not being affected by the pitching or rolling of the vessel, or that it does not interfere with any form that it may be desired to give to a vessel to insure good sailing properties, nor yet that it can be brought into action or its use discontinued at pleasure with facility, but that if the ship be going 10 knots or even more under canvas alone, at which

time the screw or the paddle would be of little or no service, it is believed that the turbine could at such a time be used with full effect, and that a speed would be obtained greater than has ever yet been realized on the ocean. Differential gear to suit the speed of the screw to the speed of the ship when under canvas, and feathering the blades of the screw to attain the same object by altering the pitch, have been tried, but have not as yet been adopted to such an extent perhaps as they may deserve; but no such alteration is required for this purpose with the turbine. The water when taken into the turbine acquires the velocity of the ship at whatever speed she may be going, and that forward motion is communicated to it quite irrespective of the circular motion and centrifugal velocity given to it by the pump; the whole, to use a technical phrase, being self-contained. Now, in the same manner as a man can throw a weight the same distance from the bow towards the stern of a ship in motion (practically speaking) at whatever speed the ship is going, so the turbine will always throw the water out from the nozzle at the velocity due to the action of the pump, and obtain from the recoil the same forward pressure, whatever the speed of the ship may be, and this pressure will produce a corresponding effect in increasing the speed of the ship. If this be correct, it will enchance the value of the hydraulic system of propulsion very much, and it seems to afford additional grounds for urging further trials. The machinery of the "Waterwitch" has been in all respects satisfactory, and reflects great credit on Messrs. Dudgeon, the manufacturers; and if a duplicate of it were to be contracted for, and put into an iron vessel of the Philomel class, which was designed with a view of being a small cruising vessel, good relative results as to speed can be guaranteed as a matter of course after the results in the Waterwitch, and an opportunity would be afforded of further trials of many kinds. When the screw was first introduced, the Rattler was kept under trial for many months, to determine the length of screw necessary for efficient effect, beginning with a long screw, and gradually reducing it with beneficial results, to the astonishment, I believe I may say of all, till it had been shortened to one-sixth of the pitch, after which the effect was found to diminish; and a further series of trials were made to determine whether screws of coarse or of fine pitch were most advantageous. The expense of these trials must have been considerable, as the ship had to be docked on every occasion of cutting the screw or putting on a different screw; but that it was a wise and prudent expenditure, was amply proved by the success of the screw ships that immediately followed; engineers having been thus enabled to proceed with confidence in designing and constructing the screws and machinery for them.

In advocating for the turbine an extension of the trials that have been commenced, these early trials of the screw may be quoted as having been of immense benefit to the country, leading to the fact of our Navy being placed on a superior footing to that of other nations for many years at that time. As evidence of the superiority, an expression of the late Admiral Sir Henry Chads, who was at first most inimical to the introduction of the screw into our men-of-war, and who was sent to cruise in the Edinburgh, block ship, to test her qualities, may be quoted, as on his return the writer of this paper heard him say to Lord Auckland, who was then Frst Lord of the Admiralty: "My Lord, if there was a war to-morrow, and you gave me my choice of all the ships in the Navy to command, I would ask for the Edinburgh."

The experiments in the Rattler were of further benefit to the country as regards the mercantile service, by enabling the manufacturers of her machinery to construct at once, with certainty, useful and profitable screw merchantmen, showing the wisdom of the Admiralty in fostering an invention that might prove an aid to national industry and wealth.

It is to be hoped, then, that the Admiralty of the present day, viewing a first expense as a true economy, may be induced to bestow the same fostering care on this new system as was done by their predecessors (amongst whom, from my own personal knowledge, I would especially wish to name Sir George Cockburn, Sir Charles Adams, Mr. Sidney Herbert, and Mr. Corry) on the screw when it was in its infancy, and follow with regard to the turbine the same enlightened and true policy of full and exhaustive experiment, and then allow it to fall or rise on its merits.

WATER SUPPLY OF GLASGOW.

From "The Engineer."

The extent to which the literature bearing upon the water supply of large cities has accumulated during the last 30 or 40 years is one of the most convincing and practical illustrations of the importance of the subject that could be adduced. Sanitary reformers and civil engineers are more and more accustomed to turn their attention to the necessity of a pure, wholesome, and sufficient water supply, and the means whereby it can be provided. When any epidemic breaks out, the quality of the water used by those infected is the first subject of investigation, and exactly in proportion as the water supply is pure and abundant, will the liability to disease be found to diminish. It is not surprising, therefore, that municipal corporations vie with each other in securing the best possible facilities for the possession of this indispensable adjunct to health and comfort; and it is gratifying to find, also, that measures are being proposed and carried out for acquiring a pure and plentiful supply, regardless of cost.

Glasgow was long destitute of anything like a proper water supply, a large proportion of water used previous to the completion of the Loch Katrine scheme, having been obtained from the Clyde—then, as now, one of the most polluted rivers in the kingdom. The evil under which the citizens of St. Mungo so long labored in this respect was all the more inexcusable, seeing that an almost illimitable source of supply lay at their very doors. Ultimately, although not for some time, that source of supply was made available, and from thenceforth Glasgow enjoyed the privilege and credit of having the most ample and efficient water supply of any town in the three kingdoms. With the circumstances which led to the projection of the Loch Katrine Water Works our readers are no doubt familiar. The magnitude of the scheme may be estimated by the fact that the Works were originally designed on a scale sufficient to supply the city with 50,000,000 gallons a day. The built portions of the aqueduct, with all the tunnels and bridges on the line, were constructed at first of sufficient dimensions to pass this enormous quantity. From the first the storage provided in the lochs was sufficient to insure the constant supply of 50,000,000 gallons a day to the city, and of 40,500,000 gallons a day of compensation water to the river Leith—making a total of 90,500,000 gallons per day. The cast-iron troughs which form portions of the bridges on the first 11 miles of the aqueduct, and the pipes across the Valleys of the Duchray, the Endrick, and the Blane, on the line of the aqueduct, were, however, put down of a size to supply only 20,000,000 gallons per day, because these portions of the works were capable of easy extension as the requirements of the city expanded. When the Glasgow Town Council first resolved in 1852 to take the water supply into their own hands the quantity of water supplied to the city by the two companies then in existence was 14,000,000 gallons per day, being somewhat less than 40 gallons per head per day. This allowance was adopted as the basis of Mr. Bateman's calculations when designing the Loch Katrine Works in 1853-4, and it was then considered to be much in excess of actual requirements. Subsequent, however, to the introduction of the Loch Katrine supply, the consumption rose considerably above 40 gallons per head per day; and so rapidly did the increase continue that in 1864 the quantity of water drawn by the city from Loch Katrine was 19,000,000 gallons a day, and the average consumption per individual was 47 gallons per day. The extension of the water-closet system throughout the city has greatly contributed to this increase of consumption; but there are other causes scarcely so apparent. Of the three largest cities in the empire, we find that London had in 1868 441,000 water-closets, or one for every six persons; Liverpool had 25,000, or one for every 24 persons; while Glasgow had 28,000, or one for every 17 persons. During the three years that have elapsed since that time we believe the water-closet system has been extended more generally in Glasgow than in either of the other two cities with which it is here brought into comparison. The great increase of consumption involved the necessity of taking measures some 4 or 5

years ago to provide an additional supply. The extension of the water-pipes to a number of outlying villages and districts operated in the same direction. Indeed, since 1860 the Water Commissioners have extended the Loch Katrine supply to Maryhill, Springburn, Thornliebank, Nitshill, Barrhead, and Harlet; and further extensions have been made to Rutherglen, Shettleston, Cathcart, and other suburbs of the city. An addition of 2½ ft. was made to the cast-iron troughs forming part of the aqueduct bridges at the time already mentioned, making the total depth of these structures 6 ft. 6 in., while the breadth is 8 ft., so that they are now sufficient to convey the full 50,000,000 gallons a day.

The following tabulated statement will show the gradually increased supply of water sent into the city from the Commissioner's works during the period in question :—

YEAR.	Quantity sent into the city daily.	Daily consumption per head.
	Gallons.	
1861....	18,610,000	42.6
1862.....	19,630,000	42.9
1863.....	20,170,000	42.3
1864.....	22,530,000	46.8
1865.....	23,060,000	47.0
1866.....	23,770,000	47.4
1867....	27,020,000	52.9
1868...	25,400,000	48.9

It will be remembered that the population had increased from 436,900 in 1861 to 518,960 in 1868, and at the present time, with a population of 550,000, the quantity of water sent into the city daily is 28,000,000 gallons, or about 50 gallons per head per day. The steady growth of consumption has called forth in several quarters animadversions as to the waste of the supply. Mr. Gale, the able engineer of the Glasgow Water Works, in a paper "On the Distribution of Water in Glasgow," read at a meeting of the Institution of Engineers in Scotland, on the 16th of March, 1864, after showing that the quantity of water used daily for domestic purposes in Glasgow was just 26 gallons per head daily in 1838, as compared with 35 gallons in 1852, and 39 gallons in 1863, proceeds to argue that the great increase of consumption of water in Glasgow is due in a great measure to waste from imperfect fittings; that, in fact, a quantity amounting to 15 gallons per head per day runs to waste without benefiting any one.

We have reason to believe that measures will be adopted before long to get rid of this extravagant waste, and thereby to reduce the water assessment of the city. It is a noteworthy fact that the average consumption of all the large cities in England, representing a population of 6,000,000, is under 30 gallons per head per day, or fully 20 gallons per head per day less than the average of Glasgow. The average consumption of the large towns of Scotland is considerably over that of the large cities in England. Edinburgh and Aberdeen average about 40 gallons; Greenock runs up to between 60 and 70 gallons; while Glasgow, as we have already indicated, comes in between with an average of 50 gallons per head daily. The waste in Greenock is easily accounted for, there being a superabundant supply for the wants of a limited population.

The consumption of water in Glasgow is increasing at such a rapid rate, that in order to meet the wants of the population it has become necessary to make further provision for increasing the supply, and tenders are now being advertised "for the laying and jointing of two lines of 36 in. cast-iron pipes from the self-acting valves near Mugdock Reservoir to a point opposite Chapelton Farm-house, near Canniesburn Toll-bar." At present there are only two lines of 36 in. pipes from Mugdock Reservoir, which is about 7 miles from the city. These pipes are calculated conjointly to deliver 25,000,000 gallons per day, being one-half the quantity of water which the Commissioners are empowered by Act of Parliament to draw from Loch Katrine. During last summer these pipes were delivering 28,000,000 gallons per day, or 3,000,000 gallons in excess of their due capacity. So that it has become necessary to lay additional pipes from Mugdock Reservoir, which will be done under the Parliamentary powers, obtained when the works were originally designed. In the mean time, the extra mains will only be carried as far as Canniesburn toll, or about half way between Mugdock and the city, leaving the prolongation of the work to be executed as the requirements of the city may demand.

It is estimated, however, that the works to be undertaken during the ensuing summer will, considering the high price of pig iron, cost not less than £50,000, while the completion of the mains to the city, which will probably be undertaken in the course of two or three years hence, will involve an additional £80,000. We may add that it will shortly be necessary likewise to lay some additional pipes across the valleys of the line of aqueduct from Loch Katrine; but this work, which will certainly not cost less than £50,000 more, can afford to wait for some years yet, even although we take into consideration the startling fact that the population of the city is increasing at the rate of 1,000 per month.

ON THE HYDRAULIC RAM.

By W. J. MACQUORN RANKINE, C. E., LL.D., F.R.S.

From "The Engineer."

The most complete body of information respecting hydraulic rams is that contained in a treatise on the subject published by Eytelwein, about 1808, and stating the results of a very long series of experiments. The conclusions at which he arrives may be summed up as follows:—Let h be the head of pressure, in feet of water above the level of the pond, against which the water is to be forced into the air vessel, H the depth of the waste-valve below top-water in the pond, Q the volume of water run to waste in a second, q the volume of water lifted per second, L the length of the feed-pipe in feet, D its diameter in inches, d the diameter of rising pipe; then the best dimensions are

$$D = V^{2.25}(Q+q); \quad L = h + \frac{2h}{H}; \quad d = \frac{D}{2};$$

the area of opening of each of the valves to be equal to that of the feed-pipe, the valves to be as light and as near each other as possible, the air vessel to have a capacity equal to that of the feed-pipe.

When these rules are observed, the efficiency, as found by Eytelwein's experiments, is given for values of $h \div H$, ranging from 1 to 20, by the following empirical formula:

$$\frac{q\,h}{Q\,h} = 1.12 - 0.2\sqrt{\left(\frac{h}{H}\right)};$$

I have found, besides, that when $h \div H$ *does not exceed* 12, the results of Eytelwein's experiments are very well represented by the following more simple formula for the *counter-efficiency* (that is, the reciprocal of the efficiency):

$$\frac{Q\,H}{q\,h} = 1 + \frac{h}{10\,H};$$

in which the second term represents this *work wasted* for each unit of useful work.

From practical trials it appears that if a feed-pipe of less diameter than that given by Eytelwein's rules is employed the length must be increased, in order that the horizontal column of water may have sufficient momentum. For instance, in a case described to me by an eminent firm of mechanical engineers the diameter of the feed-pipe was about one fourth, and the length about double, of the respective dimensions given by Eytelwein's rules.

It is probable, moreover, that when a longer and narrower feed-pipe than that given by Eytelwein's rules is employed, the work wasted is increased nearly as the length directly, and as the diameter of the pipe inversely, such being the approximate law of the resistance of pipes.

In the case just referred to, the counter-efficiency with Eytelwein's dimensions, and according to his rule, would have been 1.29. It was actually about 3.15; so that the loss of work was increased in the proportion $\frac{2.15}{0.29} = 7.4 : 1$; which nearly agrees with the before-mentioned supposition.

Extraction of Metallic Antimony.—A new method of obtaining metallic antimony from the ore is as follows: The ore is pulverized and treated in wooden vessels with hot hydrochloric acid. The antimony is precipitated from the solution by means of zinc or iron, and the precipitate washed, dried, and melted in a crucible under a covering of charcoal dust.

PNEUMATIC TRANSMISSION.

By FREDERIC CHARLES DANVERS, A. I. C. E.

From the "Quarterly Journal of Science."

The practical application of air to the transmission of carriages on land dates only from the commencement of the present century. The first idea, however, of transmitting power to a distance by means of pneumatic pressure, appears to have originated with the celebrated Denys Papin, a Frenchman, who, in 1688, described an apparatus in which a partial vacuum produced in a long tube, by air-pumps fixed at one end, caused the motion of pistons at the other end; but no record remains to prove that any steps were taken by Papin to carry his suggestions into effect so as to derive any useful practical advantage from them. The introduction of the locomotive engine naturally directed the attention of engineers and others to the subject of the provision of improved means of communication, and this desire was doubtless stimulated by the acknowledged defects of the locomotive at that time, and its reputed inapplicability for lines with gradients exceeding 1 in 100. The first person to introduce the atmosperic system of propulsion was a mechanical engineer named George Medhurst, who, in 1810, published a pamphlet on the subject, entitled "A New Method of Conveying Letters and Goods with Great Certainty and Rapidity by Air," in which may be recognized the first practical suggestions for the introduction of what is now known as the "Pneumatic System;" and it is not a little surprising to find that in this, and two subsequent pamphlets by the same author, are foreshadowed almost everything that has hitherto been discovered in connection with this subject—all subsequent inventions having reference merely to the detailed means for carrying that system into effect.

George Medhurst's first idea clearly was to employ the pneumatic system for the conveyance of small parcels only, but he subsequently suggested its application for the transport of goods of a more bulky nature. It is perhaps a pity that he did not confine his attention in the first instance to the development of his earliest ideas on the subject, and which has subsequently been proved to be the most practical method of applying his invention, viz., for the transmission of letters and small parcels. The rage of the day being, however, for improved means of communication, it is not surprising that his own ambition and the popular clamor should have caused Medhurst to endeavor to apply his invention to a purpose for which it was ill-suited. We shall not now follow the progress of the gradual rise and fall of the atmospheric railway, from the time when John Vallance, in the year 1826, constructed a model tunnel in Devonshire Place, Brighton, 120 ft. long, and nearly 8 ft. in diameter, through which a carriage was propelled by means of air-pumps worked by two steam engines, which was the first of its kind ever constructed, to the abandonment of the atmospheric principle upon the Paris and St. Germains line for the last mile and-a-half of its length, which was taken up in the year 1860, after having been in successful operation for about 15 years. From this last named circumstance it is clear that the atmospheric system is not wholly unsuited for railways under certain circumstances, the chief ground of its applicability being upon very steep inclines, such as were unsuited for locomotives. The inconvenience, however, of having different systems of propulsion upon the same railway has been the cause which has led to the abandonment of the atmospheric principle upon every railway where it has ever been tried; the general advantages, greater speed, and undoubted superiority of the locomotive gaining for it, in every case, the preference over the latter.

Thus ended all attempts to introduce atmospheric railways; but a few years before their final abandonment the adaptation of the principle for the transmission of small parcels was again revived, and it now seems likely to come into very general use, especially in connection with the Post Office and the Telegraphs. The want of some means of speedy communication between the offices of the Electric and International Telegraph Company in London, in addition to that afforded by their lines of wire, probably led to the

invention of a pneumatic tube for that purpose by Mr. Latimer Clark, and the first tube was laid down by that Company in 1855. This tube was of lead, and the carrier (in which messages were placed) nearly fitted the bore, and was covered with felt. It turned out to be a complete success, and a similar principle was adopted by the Prussian Telegraph Administration in Berlin, between the Telegraph Office and Exchange, in the year 1863; in 1866 it was also adopted in Paris in connection with the Electric Telegraph stations in that city. Later still the principle has been introduced into New York, and quite recently a line has been laid down by Messrs. Siemens, in connection with the General Post Office in London, for the conveyance of messages in original between Telegraph street and Charing Cross and on to the House of Commons, instead of sending them by Telegraph.

There are two methods which have at different times been adopted for impelling carriers through pneumatic tubes, the one being by creating a vacuum in front of the carrier, which is then impelled forward by the atmospheric pressure with a force equal to the difference between the latter and the vacuum. The other is by creating a "plenum" behind the carrier, or, in other words, increasing the pressure behind the carrier beyond that of the atmosphere, the difference between these two forces in pounds or ounces per square inch representing the force expended in driving the carrier through the tube. Medhurst, in his first invention, adopted the latter principle, and the introduction of the vacuum for the purpose is ascribed to John Vallance, who proposed it in a pamphlet published by him in 1824. Experience has shown that the vacuum is the far preferable manner of working pneumatic tubes, and it is also more economical than working with a plenum.

The difference between the effects of compression and exhaustion would appear, so far as recorded experiments upon the subject show, to vary in the cases of tubes of different diameters; but, as a general rule, it has been observed that when a carrier is inserted into a tube it is driven forwards with a mean velocity corresponding to that with which the air at the higher pressure is introduced behind it, or that at the lower pressure is exhausted in front of it. In a paper read before the British Association at Liverpool last year by Mr. Robert Sabine, that gentleman has worked out a number of formulæ for calculating the work performed in pneumatic tubes, and the result of his investigations on this subject cannot fail to be of great value, as it is one upon which very little of scientific value has hitherto been published. "The problem of a successful pneumatic system," says Sabine, "is simply this: To make a given quantity of air expand from one pressure to another in such a way as to return a fair equivalent of the work expended in compressing it. It is obviously impossible to regain the full equivalent of the work, because the compression is attended with the liberation of heat, which is dissipated and practically lost to us. Therefore, in designing a pneumatic system, that which we have to do is first to contrive means of compressing the air as economically as possible; secondly, to get back as much as we can of the mechanical effect stored up in our already compressed air, irrespectively of the work which was employed in compressing it. The utmost theoretical work which a given quantity of air can be made to perform is evidently that of expanding from the higher to lower pressure; and the mechanical effect employed in propelling a carrier and air through a given tube is therefore equivalent to that due to the expansion of a tubeful of air from the higher to the lower pressure." The speed at which a carrier travels in a horizontal tube has been worked out by Sabine, and is expressed by the following equation:—

$$s = \sqrt{2g \frac{vf - \mathrm{W}\, l\, \mu}{\mathrm{W} + \frac{w_1 + w_2}{2} v \left(1 + \zeta \frac{l}{d}\right)}} \text{ ft. per sec.}$$

But when going up or down an incline—

$$s = \sqrt{2g \frac{vf - \mathrm{W}\, l\, (\sin \alpha + \mu \cos \alpha)}{\mathrm{W} + \frac{w_1 + w_2}{2} v \left(1 + \zeta \frac{l}{d}\right)}} \text{ ft. per sec.}$$

In these equations the volume of the tube in cubic feet is represented by v; l represents the length of the tube in feet, and d its diameter also in feet; W is the weight of the carrier in pounds, and g the accelerated motion due to gravity; f represents the mechanical effect performed by 1 cubic ft. of air; μ, the coefficient of friction of motion of the carrier in the tube; w_1 the weight in pounds of 1 cubic ft. of air at the higher pressure; w_2 the weight of a cubic foot at the lower

pressure ; and a the angle made by the tube with the horizon, and which is + when the carrier ascends, but — when it descends. ζ is an empirical constant ; experiments to determine its value have been made by Girard, D'Aubuisson, Buff, Pecqueur,and others, who give a mean value for it of 0.02.

Dr. P. Brix, Professor at the Bau-Akademie, has published in the German "Telegraph Journal" particulars of experiments made by him upon velocity with a tube 2½ in. in diameter, laid down some years ago by Messrs. Siemens at Berlin, between the Exchange and Central Telegraph Station, the results of which were that when working with compression the tension of the air at either end of the tube was 19.31 lbs. and 14.75 lbs., and with exhaustion, 14.75 lbs. and 10.19 lbs., respectively; the mechanical effect produced by 1 cubic ft. of air in each case was 512.17 lbs in the former, and 520.44 lbs. in the latter ; and the weight of the air at the two extreme tensions, was, in compression, 0.1099 and 0.0753 foot-pounds, and in exhaustion 0.0752 and 0.0447 foot-pounds. For each case the frictional resistance of the carriers in the tube averaged 0.1 lb.; the length 2,920 ft. for each half of the tube ; its diameter 0.193 ft.; and its volume, 85.49 cubic ft. With these values, the formula worked out by Sabine gives the calculated speeds in these two experiments as follows :—With compression, 34.1 ft., and with exhaustion 43.2 ft. per sec., or that the carrier should have occupied in the transit from station to station, in the former case 86 secs., and in the latter 68 secs., differing from the observations made by Dr. Brix 9 secs. in the one case, and only 2 secs. in the other. This difference, Sabine thinks, may possibly be due to an error of observation of the pressure, or possibly to the fact that the constant ζ may not be the same for small welded iron tubes as for a large cast-iron tunnel.

Mr. Sabine has also made some experiments with the tube of the Pneumatic Company between Euston Station and High Holborn, which was some years ago designed by, and carried out under the engineering superintendence of, Mr. Rammell and Mr. Latimer Clark. This tube is ⊓-shaped, 4½ ft. broad and 4 ft. high. The trains used were each made up of 3 trucks, and these were loaded with an average weight of 6 tons, making, with the carriages, a gross load of 9 tons. The average time occupied in running through the tube from Euston Station to Holborn was 7½ min. with a partial vacuum of 5 oz. per sq. in., whilst the empty trucks were returned to Euston Station with a compression of 5 oz. per sq. in. in 6½ min. Assuming the temperature of the air to have been 20 deg. C., and its mean pressure 14.75 lbs., it is calculated that in drawing the loads through to Holborn the air was exhausted to 14.44 lbs., and in sending back the empty carriers it was compressed to 15.06 lbs. per sq. in. From these data the mechanical effect due to the expansion of 1 cubic ft. of air in the two experiments has been deduced to have been 31.964 foot-pounds with exhaustion, and 31.945 foot-pounds with compression.

In employing the same amount of mechanical effect, and the air remaining of the same mean specific gravity, Sabine finds "that the mean speed of transmission varies inversely with the length, and inversely also with the square root of the diameter of the tube. Thus, with an equal mechanical effect expended upon it in each case, a very light piston would travel through a tube of 1 mile long with exactly twice the speed with which it would travel through a similar tube 2 miles long. And further, if we had two tubes, each 1 mile long, one having a diameter of 4 ft. and the other a diameter of 1 ft., the air in the larger tube would only travel half as fast as that in the smaller one, assuming, of course, the total work performed during the transit to be in each case equal. The cause of this is simply that the greater portion of the mechanical effect which in the larger tube is used for moving the greater mass of air, is, in the smaller one, converted into speed. If the case arose, therefore, that a pneumatic transit had to be made with a stated expenditure of work, we should proceed economically by adopting a tube of small rather than one of large sectional area. With an equal utilized engine power in each case, the mean speeds of transit of air through two tubes are inversely as the cube roots of their diameters and lengths. For instance, with a utilized effect of 10-horse power, the velocity of transit in a tube 8 miles long, being 20 ft. per sec., that attainable with the same power in a 1 mile length of the same tube would be 40 ft., and if we had two tubes of equal length—one 8 times the diameter of the other—the speed attained in the larger tube would be only

half that attained in the former. To obtain the same speed of transit of a very light piston in two tubes of equal length and different diameters, other things being equal, the utilized horse-power must be directly proportioned to the diameter, whilst to produce the same mean speed of transit of very light pistons in tubes of equal diameter but different lengths, other things being equal, the utilized horse-powers of engines may be taken as directly proportional to the lengths. Similarly, when the lengths and diameters are equal, but the mean specific gravity of air in the two operations are different, the mean speed of a very light piston being the same in its transits through the same tube, or through two tubes of equal dimensions, the utilized engine power is directly proportionate to the mean specific gravity of the air on the two sides of the piston. It follows from this, therefore, that in working by exhaustion less engine power is required, other things being equal, than in working through the same tube by means of compression. And it would also follow that in hot weather, and when the barometer is low, the working of a pneumatic tube should be less costly in engine power than in cold weather and when the barometer is high.

"With given utilized horse-power operating upon a given line, the velocity of a very light carrier would be reciprocally proportional to the cube root of the mean specific gravity of the air moving in it. Mr. Siemens has proposed to take advantage of this fact by the employment of hydrogen gas for propulsion in letter tubes instead of atmospheric air. The specific gravity of hydrogen is 0.07; that of air being 1. The speed attainable, therefore, by the substitution of this gas would be as—

$$1 : \frac{1}{\sqrt[3]{0.07}}, \text{ or as 1 to } 2\tfrac{1}{2} \text{ nearly.}$$

This plan would be easily practicable with Messrs. Siemens' system of complete circuit tubes, in which the same air is pumped round without being changed. With any of the ordinary systems by which the tube is open at one end, of course only the atmospheric air could be used in practice."

A by no means unimportant matter in connection with the working of pneumatic tubes is the mechanical means employed for producing the vacuum or plenum, as the case may be. Several methods have been introduced with this object. The first system adopted by Vallance in his model at Brighton, in 1828, was to produce the vacuum by means of air-pumps worked by two steam engines, and this was the plan afterwards most generally employed on the several experimental lines of pneumatic railway laid down in different parts of the United Kingdom and elsewhere. In the Prussian pneumatic despatch tube both compression and expansion are employed. The tube itself consists of two tubes of welded iron 2½ in. internal diameter, laid parallel to one another beneath the pavement. A transverse coupling connects them together at one end, whilst at the other end they terminate in two reservoirs, between which an air-pump exhausts the air from one and compresses it into the other, thus keeping up a continual circuit of current within the tubes.

The pneumatic system in use in Paris differs from the foregoing principally in the use of water-power instead of steam-engine power for working the tubes, each station being supplied with an arrangement for compressing air. Until recently the transmission of carriers between stations was effected by means of compression alone, produced by the action of water upon a chamber full of atmospheric air. This water is obtained from the River Ourcq, and is employed in the following manner:—Three wrought-iron cylindrical vessels are erected at each station, one of which is large and the other two smaller, and of the same size as one another. The larger vessel is connected by means of a pipe with the water mains of the town, and an exhaust pipe leads from the same vessel into the sewers; each of these pipes is fitted with a valve to enable the communication to be opened and closed at pleasure. From the top of this vessel a pipe leads into the first of the two smaller reservoirs, and these again are connected together by a pipe fitted with a cock, whilst the second smaller reservoir is in communication with the pneumatic transmitter. In order to obtain a supply of compressed air, the valve is closed in the tube communicating with the sewers, and that leading to the water mains is opened, allowing the supply of water to rush into the larger vessel, and to displace the air which previously filled it. This displaced air is compressed into the two smaller

reservoirs until the water has risen nearly up to the top of the larger vessel. A cock, in the pipe communicating between that and the smaller reservoirs is then closed to prevent the air returning. The water is then run out of the larger vessel into the sewers, and it again becomes filled with air, which is in turn compressed into the reservoirs as before, as soon as it is required.

The Pneumatic Dispatch Company in London employ a pair of horizontal engines to drive a fan 22 ft. in diameter, by means of which the air can either be exhausted from, or forced into, the tubes at pleasure.

The latest form of pneumatic tube is that recently laid down by Mr. Siemens, from the Post Office in St. Martins-le-Grand towards Westminster, and which will probably be extended, in course of time, in other directions. Several entirely new features have been introduced in the construction of this tube, which we shall describe more fully at some future time in our chronicles of engineering, when the system has become more perfected than it is at present. One new feature which Mr. Siemens proposes to introduce is a novel kind of blower, which he has designed, to be worked direct by the steam from a boiler, without the intervention of machinery, and which will not only effect a considerable saving in the first cost of the requirements of each station, but it will also be much more economical in working and for maintenance.

The methods adopted at the different stations on any pneumatic line differ, of course, with the various methods employed for transmission, and it is in this that very great improvements have recently been introduced by Mr. Siemens. We have stated that in the Berlin pneumatic despatch system two tubes are placed parallel to each other, and connected together at one end by a transverse coupling, whilst at the other they terminate in two reservoirs, between which an air-pump exhausts the air from one and compresses it into the other, by which means a continual circuit is maintained, and provision is made for the despatch of messages in either direction. The tube for transmitting a carrier from any station is connected with the pressure reservoir, beyond which connection it is continued at a slight incline in which are placed two cocks, at an interval equal to the length of the carrier. Beyond the second cock the tube is open at the top in the form of a trough, into which the carrier is first placed. The first cock is then opened, allowing it to pass down the tube as far as the second cock. The former is then closed again and the latter opened, whereupon the carrier descends the inclined tube until it passes the pipe communicating with the pressure reservoir, whereupon it is caught by the current of air and blown to its destination. These carriers are thin metal cylinders, nearly filling the tube, supported by 4 wheels, 2 at each end, and alternately at right angles to each other. The receptor consists of a square box placed in continuation of the tube connected with the exhaust reservoir, with which it communicates by means of a cock bored to the same diameter as the tube itself. This box is lined with brushes, through which the carrier forces its way, and its impetus is thus checked, whilst at the extreme end is an india-rubber buffer. The Exchange station, at the other end of the tube, is supplied with similar apparatus, only, of course, without the engine, pump, etc.; instead of which the tubes are connected together by a short coupling tube. In some experiments which were made to determine the relative pressures in the two reservoirs, it was found that with equal differences of 9 in. of mercury above and below atmospheric pressure, the transit time of a carrier the whole distance of 3,000 ft. was 95 secs. from the station to the Exchange, whilst it only occupied 70 secs. in returning. The pressures now employed are, in the one reservoir, 7 in. of mercury over, and in the other 6 in. under atmospheric pressure. With this arrangement the transit times were, from station to Exchange 1 min. 30 secs.; and from Exchange to station 1 min. 20 secs.

In the system in use in Paris the same apparatus constitutes both the receptor and transmitter for the carriers. It consists of a cast-iron stand or pedestal, surmounted by an air-tight box, in front of which is a lid or door. Two tubes enter this box from opposite sides; one leading to the pressure reservoirs and communicating, through a cock or valve, with a branch below it, with the box, whilst the other branch from the box is connected with a tube open to the air at the top, and also provided with a cock. A central vertical tube closed at the top is used

when the carriers arrive, and acts then as an air buffer, against which they expend their force. Beneath the box another tube leads to the next station. In sending a message it is placed in a box and the door shut, the cock communicating with the compression reservoirs is then opened, and the pressure of air blows the carrier through the tube. At this time the cock communicating with the open tube is kept closed, but when a message is to be received this cock is opened and the other kept closed—the open tube admitting the escape of the air in front of the carrier.

According to Mr. Siemens' new method, a complete circuit is formed by the current of transmission, with which several stations may be brought into communication with each other. The transmitting and receiving apparatus is extremely simple, and consists of two short pieces of tubing the same diameter as the main tube, and out of the latter a piece is removed of equal length. By means of a crank, or rocking shaft movement, either of these short tube pieces may be connected with the main tube by a simple movement of a lever, and thus brought into circuit. One of these short tubes is open throughout. This is the transmitter. It is ordinarily kept in circuit, so that messages to other stations beyond may pass through. When it is desired to send a message, the circuit is broken by moving the transmitter a little to one side; the carrier with its message is then placed inside, and after communicating by signal with the station for which it is intended, the transmitting tube is once more brought into circuit, when the current of air immediately catches the carrier and hurries it on to its destination. Upon receipt of the signal at the further station for which the message is intended, the person in charge brings the receiver into circuit. This consists of a tube similar to the transmitter, with the exception that it is partially closed at one end so as to catch the carrier as it arrives. Its arrival is ascertained by the click caused by its striking the partially closed end of the receiver, which is then drawn back to extract the carrier, and the open transmitter is at the same time thrown into circuit so as to allow any through messages to pass. As each through message passes a station it causes a small bell to ring, as a signal to the superintendent, and to enable him to count when any message is due for his station. By this means a continual service of messages may be carried on in any circuit, the amount of business transacted being limited only by the means of the superintendents at each station to keep pace with the arrival and despatch of messages.

MARINE ENGINES IN THE BRITISH NAVY.

From "The Engineer."

So long as our ships of war were provided with masts and sails, the failure of their engines might be looked upon as a matter of second-rate importance. The Warrior, Bellerophon, and other ships of what may now be regarded as an old type, enjoyed peculiar advantages in this respect. In the mercantile marine we seldom or never meet with ships at once full powered and full masted. Either the engines are everything and the sail power of the ship little or nothing, or the masts and sails are regarded as the essential means of propulsion, the engines being merely auxiliaries to be used with discretion in calms and light winds. A break-down in the engine-room of a full powered Atlantic steamer leaves her practically useless for the time being, though not quite helpless. The failure of auxiliary engines, on the other hand, can do little more harm than is represented by the duration of a given voyage being possibly prolonged by a few days. Our first steam war ships, however, were not only full masted but full powered, and if one means of progression failed, the other was always available. But our new men-of-war of the type represented by the Glatton and Devastation are absolutely dependent on their engines and on their engines alone. A Cunard steamer breaking a paddle shaft in the middle of the Atlantic could still manage to get to a port, because she could show something like a fair spread of sail; but ships of the Glatton type would become helpless logs upon the water if their machinery gave way. In a word, not only the efficiency, but the very safety of the ship and crew ab-

solutely depends on the integrity of the engines by which they ought to be propelled. It has frequently been argued for this very reason that the construction of mastless ships was a great mistake; but, in reply, it has always been urged that so long as machinery was carefully protected from shot, the chance of a break down was too slight to deserve attention. This argument was good as far as it went, but men must be blind to passing events if they are content to accept it any longer. Unfortunately the breaking down of marine engines in the navy appears to be becoming the rule instead of the exception. We have recently called attention to the repeated splitting and cracking of the great cylinders of our largest iron-clads, and as if to confirm the accuracy of our statements, and to lend force to our arguments, the news comes to us this week that the Swiftsure —a very recent addition to our navy— cracked both her cylinders on her first run from Jarrow to Keyham. This is bad enough, but it is not all. The cylinders were patched, and it was believed that no further failure need be feared; but the belief was unfounded, and, as we report in another column, the cylinders have again given way and are to be patched again. There is an old sea-phrase about "patch upon patch, like a sand barge's mainsail." The new version will run "patch upon patch, like an iron-clad's cylinder." Patching a mainsail is all very well for the owners of sand barges, but we cannot look upon the presence of patched cylinders in our unmasted navy with anything like contentment. And yet the marine engineers are in this dilemma:—If they take out a pair of cracked cylinders and replace them with others precisely the same, as like causes produce like effects, it is certain that the new cylinders will quickly follow those which they replaced to the scrap heap. If, on the contrary, a different design is adopted in the new cylinders, then the makers tacitly admit that they made a mistake in designing the first cylinders, and so far they imperil their reputation; and besides, it must be remembered that the new cylinders may not be a bit more durable than their predecessors. However, whether a reputation does or does not suffer, it is clear that we cannot go on making our cylinders in the old way. There is not a pin to choose among the firms who supply our men-of-war with engines. No matter where the cylinders come from, they split, and it is evident that the cause of these repeated failures must be investigated and dealt with on scientific principles. No rule-of-thumb scheming, no putting a bit more metal here and a bit less there, as best suits the fancy of the designer, will overcome the difficulty. We must first learn all about it; and this done, it is certain that there is sufficient talent and skill available among our marine engineers to enable them either to elude the difficulty or to beat it by main force.

There is not a firm of marine engineers in this country who could not legitimately plead want of experience as their excuse for the failures. The particular kind of break-down under consideration is confined almost altogether to engines of the largest size. Of these engines really very few have been built; and it is just as well that this fact should be made to assume its proper importance. It is almost true that the largest cylinders in existence are to be found in the navy; and it is quite true that the largest horizontal engines ever built propel our war ships. The cylinders of the Hercules, for example, are 127 in. in diameter, 4 ft. 6 in. stroke. A few figures about cylinders will not be altogether out of place. The Persia had cylinders 100 in. in diameter, with a piston stroke of 10 ft. The Arabia had 103 in. cylinders, 9 ft. stroke. The Golden City—Pacific mail—had a single cylinder 105 in. diameter and 12 ft. stroke. We could, if it were necessary, easily give a long list of enormous cylinders; but it will not be disputed that engines with cylinders 120 in. in diameter are extremely rare. It is quite true, nevertheless, that as far as mere weight of metal goes, heavier cylinders than any to be met with in our navy have been successfully used for years, *but they were always fixed in a vertical position.* Engineers are, in a word, only learning now of what the horizontal type is and is not capable; and as not more than a dozen horizontal engines of the dimensions thereabouts of those in the Hercules, for example, have ever been made or used, it is certain that engineers must lack experience in the construction of such colossal machines.

It will be seen that we have placed particular stress on the horizontal position of the cylinders, and this we do because we believe that to it, and to it alone, the whole of the mischief is due. It remains to be seen whether the obstacles which exist to the use of very large horizontal cylinders can or cannot be got over; but meanwhile we have the broad fact to go upon, that whereas cylinders larger in one sense and heavier than those in use in the navy have been worked for years without failure, we find other cylinders with very much in common with the successful ones, splitting almost the moment they are put to work on their sides. Is it too much to assume that, if the cylinders of the Persia had been bolted down in a horizontal position on a rigid framework, they would have split?

We have said that it is essential to success in any attempt to get over the difficulty, that the causes of the failure should be carefully investigated, and this assertion requires a word of explanation. We assume that the most obvious cause of fracture is the position of the cylinders on their sides, and this we do simply because, as we have been at some pains to show, vertical cylinders as heavy do not break. But the horizontal position will not alone explain the causes of failure. It is only one condition of failure, one link in a chain. The reason why large horizontal cylinders split must be sought in the unequal strains set up in a great body of metal irregularly shaped by contraction and expansion. From the way in which these cylinders are fixed, the top side is always more free to expand than the bottom side, and, as a consequence, the cylinder tends to assume the form of an arch, and this will bring an enormous strain on the metal. It must not be supposed that the cylinders always crack longitudinally; on the contrary, the crack may run in any direction, usually taking its rise near the corner of a port. It is a complete mistake, again, to imagine that the cylinder is uniformly of the same temperature throughout; on the contrary, every portion of it changes its heat more or less throughout the progress of each stroke. As an illustration of our meaning, we may cite the case of Cornish engine cylinders. As they are single-acting, the top of the cylinder is always hotter than the bottom, and it is therefore the practice to bore large cylinders as much as $\frac{3}{16}$ in. smaller at the top than the bottom, so that the cylinder may be all of the same size when under steam. In the same way the cylinder of a double acting expansive engine is, no doubt, larger at each end than in the middle, on the average; but it is equally certain that it is in continual motion, expanding and contracting at each end with each stroke of the engine, while the central portion remains pretty much of the same size. Large horizontal cylinders are virtually so weak that they must be bored in a horizontal position if they are to be true. And when we tie down what are virtually huge rings of cast iron with heavy excrescences on them, to a massive framing, a prediction that they will break when heated is in strict accordance with the laws of science. The chance of failure is, beyond question, increased by the use of hard metal. It is, of course, desirable that a cylinder face should be as hard as possible; but it is not an easy task to combine hardness and toughness in the same cylinder.

More than one radical cure for the evil fortunately suggests itself. In the first place, as we have already pointed out, the best plan will be to use more cylinders than two, and so keep them of reasonable dimensions. Whether this plan is or is not adopted, a very radical change must be introduced in the design of framing, the cylinders being only secured at one end, and *supported*, but not fixed at the other. To carry out this arrangement effectually will require no small amount of talent and skill on the part of the designer. The most radical remedy of all would be the resort to vertical cylinders, and there is much less to be urged against this than may appear at first sight. In our earlier steam frigates it was essential that the machinery should be kept below the water line, to secure immunity from the guns of an enemy, but this consideration is of no weight in the case of iron-clad ships. Nothing, indeed, would be more easy than to provide, even in an otherwise unarmored ship, the most efficient protection for such of the machinery as might chance to stand 20 ft. above the keel, in the shape of a few armor plates, disposed box-battery fashion on the main deck.

EXPERIMENTAL STEAM BOILER EXPLOSIONS.

By Professor R. H. Thurston.

From "The Journal of the Franklin Institute."

The public, quite as much as professional engineers, have, from the earliest period in the history of the application of steam to useful purposes, felt a perfectly justifiable distrust of the steam boiler, whatever its form.

Indeed, the greater our familiarity with that powerful instrument, the more thoroughly do we appreciate the danger which attends its use, and those of the profession who have ever had charge of the machinery of a steam vessel, need not be reminded of the unceasing sense of anxiety and responsibility that, in most cases, has probably oppressed them by day and by night, when steaming, even where they have felt the greatest confidence in the intelligence and zeal of those to whom they have intrusted the care of the boilers.

Such terrible disasters as that which occurred on the Westfield last summer, and the epidemic of explosions that have signalized the last few months, have thoroughly re-awakened and intensified the apprehensions so universally felt.

It is to be hoped that a useful result may be a more earnest and intelligent investigation of the subject, and such additional legislation as may make the system of governmental inspection far more efficient than it is at present in the prevention of dangerous explosions.

During a few years past a number of accomplished engineers have been called, by the character of their duties, to investigate the circumstances attending nearly every case of steam boiler explosion in Great Britain, and recently many cases in the United States have been examined with similar care and skill.

A committee of the most experienced and talented among British engineers has also recently given attention to the same subject, with the object of determining what legislation should be recommended, and how far legislation may be expected to remedy this apparently rapidly increasing cause of danger to the public.

The very considerable amount of information thus obtained has been extremely useful in dispelling many of the strange superstitions and extraordinary theories, of which both ignorance and intelligent but misdirected ingenuity have been wonderfully prolific. The conclusion which has been arrived at, after the examination of many hundreds of cases of accidental steam boiler explosion, is that such so called accidents are the result of neglect or ignorance, and are never to be attributed to causes which are not, or may not be, easily understood by persons of ordinary intelligence. It is believed that most of these causes are now understood, that they are few in number and are readily controlled by the exercise of intelligence and vigilance.

A *direct proof* of these deductions is, however, still needed, and this can only be obtained by a series of experiments made with direct reference to the production of such proof. Such experiments would be the exploding of boilers under, as nearly as possible, the conditions observed in practice and carefully studying the influence that variations of those conditions may have upon the nature and intensity of the resulting effects.

Comparatively little has yet been done in this direction. More than thirty-five years ago, a committee of the Franklin Institute made a series of experiments of such extent and accuracy that the republication of their reports, and their circulation among engineers, would today be a public benefaction.* Their reports, together with the paper of F. A. Paget, on the "Wear and Tear of Steam Boilers,"† and the little book of E. B. Martin‡ on explosions, should be in the library of every engineer.

The experiments of the committee of the Franklin Institute were made upon a small scale, and upon constructions quite different in form from most steam boilers, and, although the information obtained was invaluable, it still remained desirable to repeat their experiments and to make other investigations with boilers of full size, such as are used in steamers,

* Vide Journal Franklin Institute, 1836, Vol. XVII.
† Ibid., 1865 Vol. I.
‡ Records of Steam Boiler Explosions; E. B. Martin. London: E. & F. N. Spon, 1871.

on our railroads, and in our manufactories. The only experiments of this kind ever attempted were probably those referred to by the President of the Franklin Institute, in his letter published in the concluding number of the last volume of this Journal.

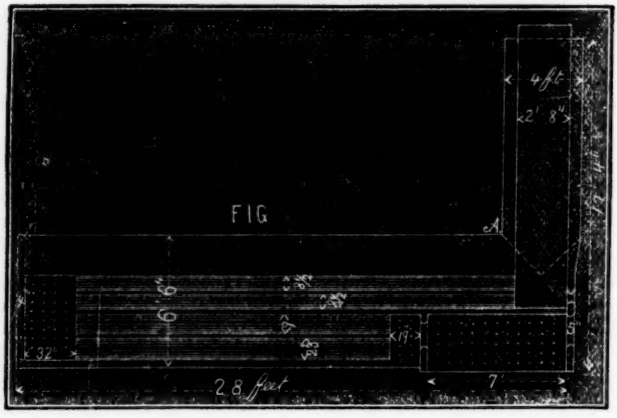
It was intended to defer an account of their orgin and progress until the completion of the series, but as cold weather has interrupted operations, and as the work will not be resumed until spring, it is considered advisable to describe the experiments already made.

These experiments were projected and conducted by Mr. Francis B. Stevens, of Hoboken. They were planned several months ago, and at the request of Mr. S. the United Railroad Companies of New Jersey, with an intelligent appreciation of the importance of such an investigation, both to themselves and to the public, appropriated the sum of ten thousand dollars to enable Mr. Stevens to enter upon a preliminary series of experiments. They, at the same time invited other railroads and owners of steam boilers to co-operate with them, and offered the use of their shops for any work that might be considered necessary or desirable during the progress of the work.

Several old boilers had recently been taken out of the steamers of the United Companies. These were subjected to hydrostatic pressure, until rupture occurred, were repaired and again ruptured several times each, thus detecting and strengthening their weakest spots, and finally leaving them much stronger than when taken from the boats. The points at which fracture occurred and the character of the break were noted carefully at each trial.

After the weak spots had thus been felt out and strengthened, the boilers were taken, with the permission of the War Department, to the U. S. reservation at Sandy Hook, at the entrance to New York Harbor, and were there set up in a large enclosure which had been prepared to receive them. This work was one of great difficulty, but it was skilfully performed, and was accomplished without accident, and the 4 old steamboat boilers above referred to, together with 5 new boilers built for the occasion, were placed in their respective positions without having been in any way injured.

Finally on the 22d and 23d of November, the experiments to be described were made. A large party of gentlemen, many of whom were professional engineers, and all of whom were deeply interested in the subject, were invited to attend by Mr. S., on behalf of the United Railroad Companies of New Jersey.



The first boiler attacked was an ordinary "single return flue boiler." Fig. 1.

The cylindrical portion of the shell was 6 ft. 6 in. diameter, 20 ft. 4 in. long, and of iron a full ¼ in. thick. The total length of the boiler was 28 ft., the steam chimney, was 4 ft. diameter, 10½ ft. high, and its flue was 32 in. diameter. The 2 furnaces were 7 ft. long, with flat arches. There were 10 lower flues, 2 of 16 and 8

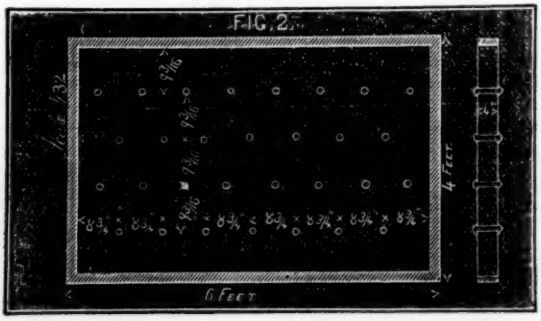
of 9 in. diameter, and all were 15 ft. 9 in. long; there were 12 upper flues, 8½ in. in diameter, and 22 ft. long. The total grate surface was 38½ sq. ft.; heating surface 1,350 sq. ft. The water spaces were 4 in. wide, and the flat surfaces were stayed by screw stay-bolts at intervals of 7 in.

This boiler was one of a pair built by Fletcher, Harrison & Co., of New York, for the steamer Joseph Belknap, in 1858, and, with its fellow, which was also on the ground, had seen 13 years of service. The last Inspector's certificate had allowed 40 lbs. of steam. The upper portion of the boiler, when inspected before the experiment, seemed to be in good order. The girth seams on the under side of the cylindrical portion had given way, and had all been patched before it was taken out of the boat. The water legs had been considerably corroded.

In September last, in presence of several gentlemen who had been invited to witness the test, this boiler had been subjected to hydrostatic pressure, giving way by the pulling through of stay-bolts at 66 lbs. per sq. in. It was repaired, and, afterward, at Sandy Hook, was tested without fracture to 82 lbs., and still later bore a steam pressure of 60 lbs. per sq. in.

On its final trial, November 22d, a heavy wood fire was built in the furnaces, the water standing 12 in. deep over the flues, and, when steam began to rise above 50 lbs., the whole party retired to the gauges, which were placed about 250 ft. from the enclosure, and which had been there proved to give accurate indications. The notes of pressures and times were taken as follows:

Time.	Pressure.
2.00 P. M.	58 lbs.
2.05 "	68 "
2.10 "	78 "
2.15 "	87 "
2.20 "	91½ "
2.23 "	93 "
2.25 "	91½ "
2.30 "	91 "
2.35 "	91½ "
2.40 "	91½ "
2.45 "	91 "
2.50 "	90 "



The pressure rose rapidly until it reached about 90 lbs.,* when leaks began to appear in all parts of the boiler, and at 93 lbs. a rent at (A, Fig. 1) the lower part of the steam chimney where it joins the shell becoming quite considerable, and other leaks of less extent enlarging, the steam passed off more rapidly than it was formed. The pressure then slowly diminishing, the workmen extinguished the fires by throwing earth upon them, and the experiment thus ended.

The second experiment was made with a small boiler (Fig. 2), which had been constructed to determine the probable strength of the stayed surface of the Westfield's boiler. It had the form of a square box, 6 ft. long, 4 ft. high, and 4 in. thick. Its sides were $\frac{5}{16}$ in. thick, of the Abbott Iron Company's "best flange fire-box" iron. The water space was $3\frac{3}{8}$ in. wide. The rivets along the edges were $\frac{3}{4}$ in. diameter, spaced 2 in. apart. The two sides were held together by screw stay-bolts, spaced $8\frac{3}{4}$ and $9\frac{3}{16}$ in., and their ends were slightly riveted over, precisely copying the distribution and workmanship of that water leg of the Westfield, which was formed between the back connection and the back end of the boiler. It had been tested to 138 lbs.

* The ultimate strength of this boiler, when new, was probably equal to about double this pressure.

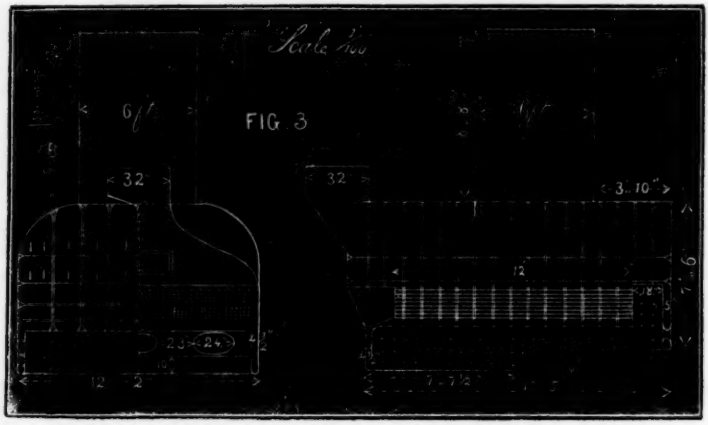
pressure. This slab was set in brickwork, about ⅘ths of its capacity occupied by water, and fires built on both sides. Pressure rose as shown by the following extract from the note-book of the writer:

Time.	Pressure.
3.18 P. M.	0 lbs.
3.20 "	4 "
3.21 "	5 "
3.22 "	7 "
3.23 "	9 "
3.24 "	11 "
3.25 "	13 "
3.26 "	15 "
3.27 "	18 "
3.28 "	20 "
3.29 "	23 "
3.30 "	27 "
3.31 "	30 "
3.32 "	34 "
3.33 "	38 "
3.34 "	44 "
3.35 "	49 "
3.36 "	51 "
3.37 "	54 "
3.38 "	58 "
3.39 "	65 "
3.40 "	72 "
3.41 "	78 "
3.42 "	86 "
3.43 "	94 "
3.44 "	100 "
3.45 "	110 "
3.46 "	117 "
3.47 "	126 "
3.48 "	135 "
3.49 "	147 "
3.50 "	160 "
3.51 "	165 "
	Exploded.

At a pressure of slightly above 165, and probably at about 167 lbs., a violent explosion took place. The brickwork of the furnace was thrown in every direction, a portion of it rising high in the air and falling among the spectators near the gauges; the sides of the exploded vessel were thrown in opposite directions with immense force, one of them tearing down the high fence at one side of the enclosure, and falling at a considerable distance away in the adjacent field; the other part struck one of the large boilers near it, cutting a large hole, and thence glanced off, falling a short distance beyond.

Both sides were stretched very considerably, assuming a dished form of 8 or 9 in. depth, and all of the stay-bolts drew out of the sheets without fracture and without even stripping the thread of either the external or the internal screw; this effect was due partly the great extension of the metal, which enlarged the holes, and partly to a rolling out of the metal as the bolts drew from their sockets in the sheet.

Lines of uniform extension seem to be indicated by a peculiar set of curved lines cutting the surface scale of oxide on the inner surface of each sheet, and resembling



closely the lines of magnetic force called, by physicists, magnetic spectra. These curious markings surrounded all of the stay-bolt holes.

The third experiment took place on the 23d of November. The boiler selected on this occasion is shown in Figure 3. It was a "return tubular boiler," with no lower flues; the furnace and combustion chamber occupying the whole lower part. Its surface extended the whole width of the boiler, thus giving an immense crown sheet, which was perfectly flat, and was braced to the shell by "crow-foot" braces

whose rods were in section 2 in. by ½ in., and spaced 12 in. lengthwise and 17 in. crosswise the boiler; each brace sustained an area of 204 sq. in. The water legs were secured by stay-bolts of 1 in. diameter, spaced 12 in. by 8. The horizontal braces were spaced 28 in. by 12, and were 1⅛ in. diameter. The shell was of No. 3 iron, single riveted. There were 384 tubes, 2 in. diameter and 12 ft. long. The steam drum was placed at the middle of the boiler, and was 6 ft. diameter, and 8 ft. 8 in. high.

This boiler was built in 1845, by T. F. Secor & Co, and had been at work *twenty-five years;* when taken out, the inspector's certificate allowed 30 lbs. of steam. In September it was subjected to hydrostatic pressure, which at 42 lbs. broke a brace in the crown sheet, and at 60 lbs, 12 of the braces over the furnace gave way, and allowed so free an escape of water as to prevent the attainment of a higher pressure. The broken parts were carefully repaired, and the boiler again tested at Sandy Hook at 59 lbs., which was borne without injury, and afterward a steam pressure of 45 lbs. left it still uninjured. At the final experiment, the water level was raised to the height of 15 in. above the tubes, and it there remained to the end. The fire was built, as in the previous experiments, with as much wood as would burn freely in the furnace, and the record of pressures was as follows:

Time.	Pressure.	
12.21 P. M.	29½ lbs.	
12.23 "	33½ "	
12.25 "	37½ "	
12 27 "	41 "	
12.29 "	44½ "	
12 31 "	48½ "	
12 32 "	50 "	brace broke.
12 33 "	52 "	
12.34 "	53½ "	exploded.

When a pressure was reached of 50 lbs. per sq. in., a report was heard, which was probably caused by the breaking of one or more braces, and at 53½ lbs., the boiler was seen to explode with terrible force. The whole of the enclosure was obscured by the vast masses of steam liberated; the air was dotted with the flying fragments, the largest of which—the steam drum—rising first to a height variously estimated at from 200 to 400 ft., fell at a distance of about 450 ft. from its original position. The sound of the explosion resembled the report of a heavy cannon. The boiler was torn into many pieces, and comparatively few fell back upon their original position.

The same bulging of stay-bolted surfaces that was noticed in the preceding experiment was observed here, and the screw stay-bolts slipped out as before, without breaking and without stripping their threads. The braces were usually broken at the welds.

Having briefly described these experiments, it may be well to notice what bearing their results have upon existing beliefs, and how far they extend our knowledge of the causes and conditions of explosions.

In the first experiment, we probably have an illustration of by far the most usual behavior of steam boilers, when yielding to over-pressure. The pressure gradually rising, ruptured the boiler at its weakest point, which happened to be a spot of merely *local* weakness; the rent extended toward stronger portions, but soon became large enough to discharge the steam as rapidly as it was made. The strength of the metal in the direction of the line of fracture being sufficient to resist further extension at the maximum pressure attained, no greater injury was done. The spot being patched, the boiler is probably still capable of doing good service for a considerable length of time.

When boilers give way from excessive weakness or from over pressure, they very generally do so in the manner described. The explosion is the exceptional case, and the frequency with which old boilers "blow out" in every part, though usually about the stayed surfaces, and the apparent impunity with which they are kept at work after being frequently patched, has probably been the most influential cause of the existence of the belief, which is, unfortunately, widespread among engineers, that the mere pressure of steam cannot cause explosions. and that, if the boiler contains a sufficient quantity of water, it is perfectly safe, except against sundry mysterious forces, which are probably, like the fairies and ghouls of earlier times, existent only in the imaginations of those whom they terrify.

In the second and third experiments, we have illustrations of the comparatively rare cases in which explosions actually occur.

The second was a perfectly new con-

struction, in which corrosion had not developed a point of great comparative weakness, and the edges yielding along the lines of riveting on all sides simultaneously and very equally, the two halves were completely separated, and thrown far apart with all of the energy of unmistakable explosion, although there was an ample supply of water, and the pressure did not exceed that frequently reached in locomotives and on the Western rivers, and although the boiler itself was quite diminutive.

The circumstance of the drawing out of the stay-bolts without breaking and without stripping their threads, was one of the most interesting points of the experiment.

Constructing a formula upon the very probable hypothesis that this was an example of average American practice, we obtain for ordinary use $d = \frac{365\,t}{\sqrt{f\,P}}$; where d = the distance between the stay-bolts, t = the thickness of the plates, P = the pressure and f = the factor of safety, which certainly *ought in no case to be less than six.*

Also $P = f\left(\frac{365\,t}{d}\right)^2$, the units of measure being lbs. and inches.

Fairbairn proved, by experiment, that the diameter of screwed stays should be double the thickness of the sheet, in order to make their tensile strength equal to the force that would draw them out of the sheets. To this should be added $\frac{1}{4}$ in. allowance for corrosion. These stay-bolts should have been $\frac{7}{8}$ in. in diameter, the $\frac{1}{4}$ in. excess of diameter being comparatively valueless.

The spacing of the stay-bolts in a boiler of such workmanship, intended to carry 40 lbs. of steam, and taking six a sa factor of safety, should, however, have been $d =$

$$\frac{365 \times \frac{7}{16}}{\sqrt{6 \times 40}} = 7.3 +,$$

or about $7\frac{1}{2}$ inches.

Fairbairn showed that properly riveting over the ends, increased the strength of stay-bolts 14 *per cent.*

In the third experiment, as in the second, it is probable that the weakest part extended very uniformly over a large part of the boiler, either in lines of weakened metal, or over surfaces largely acted upon by corrosion. Immediately upon the giving way of its braces, fracture took place at once in many different parts.

In this example, the boiler had been standing a week with steam up, but with none blowing off, and feed being pumped in, unless to supply the insignificant waste from leakage. It was set on solid ground, and the water which it contained could not have been in the slightest degree agitated. It has been a question whether the water might not, under these circumstances, have become super-heated, as in the cases first noticed by M. Deluc, and since investigated by MM. Downy, Dufour and others, and whether the violence of this explosion may not have been largely due to such action.

When it is remembered, however, that those experimenters found it difficult to induce this condition in metal vessels with even minute quantities of water, and that the extent of this super-heating becomes quite small with very small quantities of the fluid, growing rapidly less as the bulk of water increases, it may be very much doubted whether it would be possible to obtain such a state in this case, where tons of water were contained in a rough metal vessel, and also where a circulation was constantly kept up by a fire at one end.

The quantity of heat thus stored up must have been very small, even if there were any such excess.

Were it known precisely to what height the steam drum, for example, was thrown in this case, it would be easy to determine with great certainty, whether the steam released at $53\frac{1}{2}$ lbs. pressure was sufficient to produce the effects noted.

We have made such an estimate, but the data are too unreliable to admit of its publication, although it confirms our opinion that no super-heating of the water in the boiler took place.

We may conclude, then, from the result of Mr. Stevens' experiments:—

First, That "low water," although undoubtedly one cause, is not the only cause of violent explosions, as is so commonly supposed, but that a most violent explosion may occur with a boiler well supplied with water.

This was shown on a small scale by the experiments of the committee of the Franklin Institute above referred to.

Second, That what is generally consid-

ered a moderate steam pressure may produce the very violent explosion of a weak boiler, containing a large body of water, and having all its flues well covered.

This has never before, we believe, been directly proven by experiment.

Third, That a steam boiler may explode, under steam, at a pressure less than that which it had successfully withstood at the hydrostatic test.

The last boiler had been tested to 59 lbs., and afterward exploded at 53½ lbs. This fact, too, although frequently urged by some engineers, was generally disbelieved. It has now been directly proven.*

There can now be no excuse for the implicit confidence so generally felt in the use of the hydrostatic test, or for not at least combining with it, in every inspection, the use of the "hammer test" by experienced inspectors.

It may be finally remarked, that welded boiler braces, and screw stay-bolts which have not nuts at their end or are not well riveted over, should evidently be distrusted.

This extremely interesting and important series of experiments will be continued on the return of warm weather, and it is hoped that Congress may be induced to make provision for its extension. A very excellent report, made to the Secretary of the Navy, by Chief Engineers Isherwood, De Luce and S. Albert, has been printed by Mr. Stevens, together with a memorial, asking Congress to take action in this matter, which is of such great importance to the public.

These will be circulated among those who may be willing and able to aid in the plan proposed.

It is proposed by Mr. Stevens in the experiments succeding those now prepared, to determine the conditions of explosion with low water, to examine into the effect of opening the safety valve suddenly upon an already heavily strained boiler, and to check those experiments that seem to prove explosions to occur from excessive pressure, by leading steam at high pressure into a comparatively weak boiler containing no water. It is intended to explore this wide field of research as thoroughly as available time and money will allow.

It is to be hoped that our wealthy railroad corporations, and owners of steam vessels, may see how deeply their own interests are involved in the prosecution of such researches, and that they, as well as the General Government, may assist in continuing these investigations.

* A number of instances of this kind, though not always producing an explosion, have been made known to the writer.

Two boilers at the Detroit Water Works, in 1859, after resisting the hydrostatic test of 200 lbs. with water at a temperature of 100 deg. Fahr., broke several braces each at 110 and 115 lbs. steam pressure respectively, when first tried under steam.

The boiler of the U. S. steamer Algonquin was tested with 150 lbs. cold water pressure, and broke a brace at 100 lbs. when tried with steam.

A similar case occurred in New York, a few years ago, and the boiler exploded with fatal results.

These accidents are probably caused by changes of form of the boiler, under varying temperature, which throw undue strain upon some one part, which may have already been nearly fractured.

THE WAR DEPARTMENT REPORT ON GUN-COTTON.

From "The Engineer."

Public opinion, bearing upon the majority of manufactures, displays its wisdom in keeping steadily in view, as a purpose to be achieved, the rendering of manufactures harmless to those engaged in them. Mostly this can be done, or a point of perfection arrived at corresponding to nearly done. But some manufactures may be cited to which the observation does not apply, manufactures wholly exceptional, the conduct of which can neither be brought within absolute safety to those engaged, nor, as an alternative, abandoned, seeing that the result of many such manufactures represents an improvement. Gunpowder is in this category, and, in a way still more pronounced, a long list of chemical explosives, some too violent for the best control, others, though terrible, still less violent, and, under certain risks, to be calculated and dared, utilizable. Despite the best precautions observed, or capable of being observed, gunpowder mills and storages blow up from time to time; in like manner mercury fulminate factories, not to speak of dynamite, nitro-glycerine, and its congener, known as lithrofacteur. Nobody ever

proposes abandonment of the manufacture of gun-cotton or mercury fulminate on hearing of an accident; in Germany both dynamite and nitro-glycerine are made both on the large scale, without the Government—so stringent when deemed needful—having seen fit to put the manufacture down. But when in the late summer of last year an explosion of gun-cotton occurred at Stowmarket, a section of the public, scared and excited, raised clamors for the Government to step in, preventing the manufacture of gun-cotton in bulk in this country at all. And on what ground? Because of the terrible nature of the substance, and the accident, said the public; because of the large number of killed and wounded; because gun-cotton when made and stored is known to explode spontaneously sometimes, and when not exploding spontaneously is known not to keep without deterioration, or it may be utter spoliation. As to the two first reasons, they positively have no weight at all as an indictment against the commercial manufacture of gun-cotton. Like other explosives, the preparation of it on the large scale, is obviously incompatible with the manufacturer arriving at that point of absolute safety which philanthropy would desire. To be an explosive means being a source of instantaneous danger; for dependent on the instantaneous elimination of force is the pure projectile function. The substance is terrible, is known and meant to be terrible. An explosion of it occurring in proximity to human beings must well-nigh of necessity prove destructive to human life. But the third and fourth allegations, namely, that gun-cotton had been known to explode spontaneously, and that it could not be kept unchanged over a practically short space of time—these allegations, once satisfactorily made out, would, or at least should, have led to the abandonment of the gun-cotton manufacture. With respect to so-called spontaneous explosion, we have already maintained in these columns that the phrase, though very usual, is self-contradictory. Until the recognition is granted of an effect without cause, there can be no such thing as spontaneous explosion or combustion. All that the word "spontaneous" really in this application of it can be held to signify is the happening of a result determined by an hitherto occult cause. Practically, however, it matters not. Were it demonstrated or demonstrable that gun-cotton was explodable under the influence of conditions unknown, or, if known, irremediable, then, for this reason alone, the manufacture of it should be abandoned, on the very sufficient ground of being unfitted to fulfil the purpose for which its manufacture was carried on. If, again, demonstration should be made that gun-cotton could not be preserved without change over a competent space of time within the bounds of practical need, then, likewise, its manufacture would have been useless. A thing so circumstanced would have been totally unfitted for all ends required of an engineering explosive. Happily a Government Commission of Inquiry has been issued, and is satisfactory. Dealing with the two allegations, it has disposed of them in the negative, and, consequently, in a sense favorable to the resumed manufacture and engineering employment of gun-cotton. "The Committee, after a careful review of the documents in their possession, and of the evidence of the officers above mentioned and others, respecting the use and application of compressed gun-cotton, principally as regards its employment for military purposes, consider that its use is not only unattended by either uncertainty or peril, but that the material as an explosive agent is effective, certain, safe, portable, and easy in employment. The Committee therefore feel that they are warranted in the expression of a strong opinion of its great value for military engineering purposes generally, and for submarine mining. The Committee therefore feel no hesitation in recording their opinion that there is no reason why the War Department should relinquish the manufacture of compressed gun-cotton. The only part of the manufacture of gun-cotton to which danger attaches is the drying. Up to that stage the material contains from 15 per cent. to 20 per cent. of water; but the Committee express an opinion that the operation as carried on at Stowmarket is open to objection, and that no difficulty will be experienced in devising a safe and simple method of drying, which may be easily applicable to any locality." One of the most valuable points comprised in the Government preliminary report, is concerning the durability of gun-cotton when manufactured and stored. The War De-

partment has in its possession specimens of gun-cotton 9 years old, and still as good as ever. Now 9 years is, practically considered, a long time—quite long enough to warrant continuance in the use of a material the engineering utility of which, in its unchanged state, is no longer a question. Nothing in the evidence tendered goes to show that the gun-cotton already 9 years stored might not remain uninjured 90 or even 900, which is much more than can be averred of gunpowder, a material far from indestructible when quite dry, and rapidly destructible when moistened; whereas gun-cotton may not only be flooded with water without disadvantage, but with absolute immunity from change, so far as experience has gone.

In expressing satisfaction that the War Department has seen fit to make this useful investigation, and that the issue of it is so reassuring, we only echo the testimony of all who have experienced the power of gun-cotton for its own legitimate use, when we say it would have been a subject of deep regret had the manufacture of this agent been summarily abandoned in deference to unreasoning public clamor. Considered as a projectile agent for gun charges, ordnance, or small arms, we believe gun-cotton to be practically useless, and this is asserted under full acquaintance with the fact that a gun-cotton battery of field pieces was set on foot under the auspices of Baron Lenk, he who introduced the substance to Messrs. Prentice, at Stowmarket, and induced the commencement of the factory which blew up last year. Whether as a charge for artillery or small-arms, gun-cotton has invariably proved too immediately shattering for the due exercise of propulsive force, combined with safety to the gun. But the time, if it has not yet come, is near at hand when naval ordnance at least are to be of less account than formerly. Obviously, to common apprehension, the most promising way to demolish a floating structure is to hit her somewhere below the line of floatation. Ordinary artillery cannot effect this. Submarine artillery may do so, but, all evidence taken into account, a propulsive torpedo would accomplish this result better. Now, the requisite for the best torpedo explosive is not the same as for an ordinary fire-arm propulsive, where extreme shattering power is, as already stated, a defect. For a torpedo charge, the more immediately shattering an explosive is, the more *vis viva* it has, by so much is it better to promote the end desired. Of course the use of it must be brought within the limits of safety, or else it could not be employed. Gun-cotton is demonstrated to be within those limits, and so far as we know, only gun-cotton. Chloride of nitrogen would be wholly unmanageable, and, as for nitro-glycerine, it is only just one shade more practicable. In the interest of our future torpedo service, then, we say it is fortunate that the War Department Commission on the practicablity of gun-cotton has been instituted, has delivered its preliminary report, and that the report is so reassuring.

PRESERVATION OF WOOD FROM DECAY.

By HERMAN HAUPT, C. E.

Written for Van Nostrand's Magazine.

On the occasion of the proposed formation of a Company in Philadelphia for the construction of wood pavements, the writer was requested to examine and report upon the various processes employed as proposed, for the treatment of wood as a protection against decay. Considerable time was spent in this investigation, and various experiments were made, the results of which may possibly be of some interest and value to the engineering profession. Facts are generally important, and failures often indicate the road to success. The report in the processes examined was unfavorable; the Company did not go into operation, and the results of the experiments and inquiries are not generally known. The report was suppressed, as facts were reported impartially, without regard to their effect upon private interests.

No other apology will be offered for this communication, which will include portions of the report referred to. It is

believed that although all previous attempts to preserve wood by processes at the same time effective and economical, including the experiments of the writer himself, have been failures, it is not impossible to accomplish an object so desirable, and an effort will be made to indicate the conditions essential to success.

To present in an intelligible manner the processes used and results obtained in the treatment of wood, to expose the defects of existing modes of operation, and to lead to suggestions of improvement, it is neccessary to enumerate some of the principal agents and materials employed in such processes and define their properties.

WOOD.

Wood contains about 4 per cent. of soluble and 96 per cent. of fibrous material. It forms a hard porous tissue, the size and number of the pores varying with the kind of wood. The fibrous portions of all woods are heavier than water, and have a specific gravity about equal to the fibre of flax, or 1.5. The fibres are not continuous throughout the stick, but are short and overlap, arranged in echelon. Wood floats in water consequently by virtue of the air confined in the cells. When completely saturated with water, all woods must sink. Dried at 86 deg. Fahrenheit, 1 lb. troy of wood yields, by distillation, 7 oz. wood acid, $1\frac{1}{4}$ oz. combustible oil, and $3\frac{3}{4}$ oz. charcoal. The variation in different woods is not very great.

Experiments were made by Mr. Merrill, at South Boston, to determine the quantity of coal oil absorbed by wood. Pieces of green birch were boiled in oil. when the temperature rose above 212 deg., bubbles escaped in great numbers from the ends of the sticks, and in small quantities also from the sides, showing that the expulsion of water did not proceed from the ends only. At 300 deg. the bubbles ceased. Upon removing from the fire, the wood, which had floated on the surface of the oil, sank to the bottom. The specific gravity of the oil was 1.025 deg.

A second experiment was made, after carefully drying the wood, at a temperature of 286 deg., to expel all free moisture. When boiled in the oil, bubbles were produced, but not as copiously as before, which Mr. Merrill explained by the supposition that the oil had effected decompositions, and liberated portions of the water of combination.

In the second experiment, the absorption of oil was less than in the first, for the specific gravity of the pieces operated on was exactly equal to that of water, while those first treated became heavier than the oil. This observation is important in its bearing upon the treatment of wood. Green timber was more completely saturated than the dry, and the explanation is very obvious. Water, when heated to 212 deg., expands 1,700 times, while the expansion of air in 100 deg. is only $\frac{1}{30}$ of its volume. It follows from this, that if the cells of the wood are filled with air, an increase of temperature, from 60 deg. to 212 deg., will expel $\frac{56}{100}$, or about one-half, while, if the cells are filled with water, the quantity expelled, by being converted into steam, will be thirty-four hundred times as great.

It was also found that the saturated wood was more than double the weight of the dry wood, that the quantity of oil absorbed by white birch was 3.75 gallons per cubic ft., and its weight 32 lbs., at a cost for the oil only of 34 cents. At this rate, the oil required to saturate a crosstie would cost 90 cents, exclusive of cost of process.

Other kinds of timber are more porous, and would absorb much larger quantities.

It would appear also from these experiments that if the white birch be supposed to have been completely saturated, the volume of the cells slightly exceded the volume of the solid fibre, or that more than half the wood consisted of cavities.

WATER.

Water plays a very important part in all the operations connected with the preservation of wood. As a solvent it reduces metallic salts to the condition of fluids, and allows them to penetrate the cells, if previously emptied; but when the cells are saturated with moisture, nothing else can enter them until it has been removed.

Water is fluid at temperatures between 32 deg. and 212 deg.; but below 32 deg. it becomes solid, and above 212 deg. it

becomes in the open air steam. In the conversion of water into steam, a large amount of heat becomes latent. One part by weight of steam at 212 deg. in becoming water at 212 deg. would part with heat enough to raise an equal weight of water 972 deg. This is equivalent to raising $5\frac{4}{10}$ times its weight of water from the freezing to the boiling point.

One lb. of steam will raise 3,657 cubic ft. of air 10 deg., and cause it to expand from 32 deg. to 42 deg. to 3,733 cubic ft.

The heat that would raise 1 lb. of water 1 deg. would raise 1 lb. of air 3.7 deg., and 1 lb. of air = about 11 cubic ft.

Athough water boils at 212 deg. under the ordinary pressure of the atmosphere, the boiling-point may be lowered to any extent in a complete or partial vacuum, and may be increased to any extent by confinement in strong vessels under pressure. Thus water may boil at a temperature less than blood heat, and may be restrained from boiling at a heat of several hundred degrees.

The temperature of steam varies with the pressure. At 212 deg. the pressure is 14.7 lbs. per sq. in.; a pressure of 50 lbs. corresponds to a temperature 283 deg.; 100 lbs. to 332 deg.; 150 lbs. to 363 deg., and 200 lbs. to 387 deg., including the pressure of the atmosphere.

These properties will be found important in discussing the phenomena connected with the treatment of wood.

PRODUCTS OF THE DISTILLATION OF TAR.

Much confusion exists in the use of the names applied to the products of the distillation of tar. They are often misapplied, and the properties of one substance are attributed to others entirely dissimilar. In considering the treatment of wood by oils or vapors, it is very essential that their properties and the conditions under which they exist should be clearly defined.

Specifications of patents sometimes refer to the eupione and paraffine of coal oils, in which these substances do not exist, and other terms are similarly misapplied.

It is necessary to explain therefore that the products of the distillation of tar are very numerous, and embrace a wide range of hydrocarbon oils and vapors capable of uniting with each other in every proportion, but differing greatly in their boiling points and other characteristics. Some volatilize at a very low temperature, and others at a very high one. In the process of distillation every slight increase of temperature will change the character of the product, but the most careful and repeated distillations at fixed temperatures fail to secure perfect uniformity; the result is, more or less, a mixture of different hydro-carbons; and it is a property worthy of particular notice that oils which boil at lower, carry with their vapors others which boil when alone only at higher temperatures.

It does not follow, therefore, that because certain products of distillation have a high boiling-point they will not pass over with vapors of a lower temperature; of course, they cannot be so abundant as at the temperature of their proper boiling-point, but may, nevertheless, be found in appreciable quantities.

These vapors resulting from distillation cannot be maintained in this form at a temperature below that required for their production, but, upon cooling even to a slight degree, are condensed into liquids; an observation of much importance in connection with the treatment of wood.

Coal tar and wood tar differ essentially in their constituents, and these again according to the kind of coal or wood from which they are produced. The following table will exhibit the more important products, so that their properties may be compared.

Products of distillation of

WOOD TAR.

Eupione.

Thin, colorless, aromatic odor.
No taste.
Density 0 655, being the lightest liquid known.
Boils at 116°.
Inflammable.
Insoluble in water.
Composition $C_{5}H_{6}$.

Kreosot.

Fluid at ordinary temperatures.
Water dissolves 1¼ per cent.
Colorless, viscid.
Smoky odor.
Density 1.037.
Boils 397°.
Strongly antiseptic.
Combines with alkalies.
Composition, supposed, $C_{14}H_{8}O_{2}$.

Paraffin.

Solid, without taste or odor.
Volatile without decomposition.
Burns with white flame.
Insoluble in water.
Specific gravity 0.870.
Boils 700°.
Composition $C_{30}H_{30}$.
Melts at 110°.

Products of distillation of

COAL TAR.

Benzole or Benzine.

Thin colorless, peculiar odor.
Sweetish taste.
Density 0.885, and consequently heavier than eubione.
Boils at 176°.
Inflammable.
Insoluble in water.
Composition $C_{12}H_{6}$.

Carbolic Acid.

Solid when pure.
Slightly soluble in water.
Melts at 95°.
Smoky odor.
Density 1.065.
Boils 370°.
Antiseptic in the highest degree.
Combines with alkalies.
Composition $C_{12}H_{5}O, HO$.

Naphthalin.

Solid, faint peculiar odor and taste.
Volatile, without decomposition.
Burns with red smoky flame.
Insoluble in cold water; very slightly soluble in hot water.
Specific gravity 1.048.
Boils 413°.
Composition $C_{20}H_{8}$.
Melts at 176°.

It will be understood that these are not the only products of the distillation of tar, but they embrace those most worthy of notice. There are in addition various oils and vapors which are more or less mixtures, and also a residuum of pitch.

As the object of the investigation was chiefly to determine the efficacy of certain processes for permeating wood with the vapor of coal oil, it is proper that this subject should receive a more extended discussion; but as other processes are intimately connected therewith, it would seem desirable that a brief description of them should not be omitted. The undersigned proposes, therefore, to consider the subjects under investigation in the following order:

1. The application of the vapors of coal tar to the prservation of wood—the defects of existing modes and proposed improvements.

2. Other modes of preventing decay.

COAL TAR VAPORS FOR THE PRESERVATION OF WOOD.

The application of the vapors of coal tar for the preservation of wood is not new. In 1835 a patent was granted to Frantz Moll in England, and published in 1836, for a process which consisted substantially in placing the wood to be treated in an air-tight chamber heated by means of steam pipes, or "*otherwise*," to a point sufficient to prevent condensation of the vapors, and the distilling over from retorts, producing what he calls eupione and kreosote, but which should have been called, according to the recognized chemical nomenclature, benzine, carbolic acid, and naphthalin. The claim did not cover any particular form of apparatus or mode of producing the vapors, but the use of such vapors, by whatever name they may be called, applied in that form to prevent decay. It is obvious, therefore, that no patent granted subsequently in the United States claiming the same invention can be valid.

KIND OF OIL USED.

The oil used in the treatment of wood is known at the distilleries under the name of *dead oil*. It is a product of the distillation of ordinary gas tar. At the works at South Boston 40 barrels of tar are placed in 2 stills, from which pipes proceed to a worm contained in a large wooden tank of cold water. The vapors which first pass over, consisting principally of benzine, condense into about 2 barrels of this fluid, which floats on water, and is removed from the vat into which it flows. Next about 10 barrels of a heavy fluid (dead oil) passes over, which has a specific gravity of 1.020 to 1.030, water being 1.000. The contents of the still are then run into another vat, and constitute roofing pitch, the quantity obtained from a charge being about 27 barrels out of the 40. The benzine sells at 25 cts., the dead oil at 9 cts., and the pitch at 13 cts. per gallon. In England dead oil is worth only one penny per gallon. At 9 cts. per gallon, about 1 cent per lb., dead oil is the cheapest known material for the preservation of wood, and is also the best. It contains variable proportions, but usually about 20 per cent. of carbolic acid.

EXPERIMENTS AT BOSTON.

Experiments were made in Boston with a suitable apparatus to determine the effect of subjecting wood to a bath of heated vapors of dead oil. The apparatus consisted of a tank of boiler iron, 13 ft. long and 4 ft. in diameter. The front end was a heavy casting, 2 in. thick, strengthened by flanges on the inside,

held in place by a hinge and supported by a roller running on a plate of iron. The door was packed with a ring of rubber, and secured by 16 bolts to resist the internal pressure. Steam pipes passed into the tank from the rear, and connected with a boiler used for driving the machinery of the mill. The bottom of the tank was filled with pipes through which the steam circulated for the purpose of evaporating the oil. Another pipe opened into the tank through which steam could be admitted when necessary. On top were a thermometer, a pressure gauge, and a vacuum gauge. A low partition, 9 inches high, of boiler iron was placed 6½ in. from the door, across the bottom of the tank, to retain the oil.

One barrel of oil was carefully weighed —405 lbs. It was then emptied into the tank; the wood was introduced, the door closed, and then heat was applied by steam at 80 lbs. pressure above the atmosphere, the temperature due to which pressure being 328 deg. A bent tube passing into a bucket of water was placed on top of the tank, to indicate by the bubbles when the air was expelled and the oily vapors filled the space.

The wood consisted of 24 pieces—spruce, pine, and walnut—part green and part dry. The dry wood, in addition to two years' seasoning in the shop, was baked 15 hours in an oven to insure the expulsion of all free moisture. Each piece was numbered, marked, and weighed, and the total weight found to be 1,590 lbs.

The timber was exposed to the vapors from 10.30 A. M. to 4 P. M., 5½ hours; then upon cooling for 5 hours the vacuum gauge indicated 2 lbs. per sq. in. The steam was again turned on at 9 P. M., and remained until 4 A. M., during which time the wood was exposed to the vapor bath, and the thermometer at the top of the tank indicated 149 deg.

At 10 A. M. the tank was opened. The interior of the tank was coated with large crystals of naphthaline. The space in front of the partition was filled with water, but there was no water whatever on top of the oil. About 46 lbs. of water had escaped from the green wood. The oil had lost only 10½ lbs.; the dry wood had gained 16 lbs. The green wood exhibited no evidence whatever of any permeation by oily vapors; the dry wood very little. The experiment proved that the mode of treatment adopted was entirely worthless, as might have been inferred from the insufficiency of the heat employed, and there was no possibility of increasing it with that apparatus except by the addition of another boiler to provide superheated steam.

A second experiment was made at the suggestion of parties connected with the works, which consisted in exposing the timber for some hours to the action of steam mixed with vapor of dead oil, but the results were not satisfactory. It is proper to observe, however, that samples of paving blocks were exhibited which had been treated by the steam and vapor process, in which crystals of naphthaline were found in the interior, showing a certain degree of penetration, but the condition of the blocks as to dryness before being subjected to the process, could not be ascertained. It is not impossible that in certain conditions this mode of treatment might secure sufficient permeation to increase the durability of short pieces like paving blocks.

OBSERVATIONS.

It is well known that the very small portion of kreosote contained in smoke, will penetrate to the interior of a ham, and cure it. The soluble portions of wood are only 4 per cent. of its weight. If, as high authorities assert, the five-thousandth part of carbolic acid will prevent putrefaction of blood, fæces, glue solution, flour paste, and other substances liable to fermentation, the same proportion should protect the albuminous and soluble portions of wood. And if this be true, only the one hundred and twenty-five thousandth part of the weight of the wood would be required. This is less than the two hundred and fiftieth part of an ounce to a cubic foot, and more than this may be conveyed into paving blocks by the vapor process. While it seems probable that thorough saturation with oils, as in the Bethel process, is unnecessary, a much larger portion than the minimum required to prevent putrefaction would seem to be desirable.

Whatever course of treatment be adopted, or form of apparatus used, certain conditions must be fulfilled to secure satisfactory results, and their consideration will indicate the changes in present

modes of operation which appear to be essential to success, for these conditions have not been fulfilled by any processes or plans of operation, that have ever yet been adopted in the treatment of timber.

1. The required condition of the timber as to elasticity must be considered, and it must depend on the uses to which the timber is to be applied. In many situations, as for posts, rigidity is not objectionable, and the impregnation of the cells with resins, or other brittle solids, may give satisfactory results. In other cases, elasticity is as essential as durability. No treatment that would render timber brittle would be applicable to bridges, or beams to sustain weights. Even cross-ties must be elastic. If rendered rigid by the introduction of any substances, with a view to prevent decay, a more rapid destruction ensues from the percussive effects of the trains than from ordinary rot. Dr. Cresson stated very correctly, in conversing on this subject, that the substances introduced to preserve wood for cross-ties must be in a permanently *gelatinous* condition. There must be nothing to prevent that movement of the fibres upon each other, on which the elasticity depends.

Similar conditions are required in the treatment of paving blocks. The original status of the fibre must be maintained. If rendered brittle, the ends of the fibres will break off, and abrasion become rapid. The gelatinous condition is, in this case also, the true one.

2. The condition as to moisture and dryness of the timber will also modify the processes employed. Green timber is often completely saturated with moisture. Its expulsion by ordinary agencies is a very slow operation. Exposed to currents of air, even boards will not become dry in a year. It is an axiom that two substances cannot occupy the same place at the same time; and before any vapors or fluids can permeate the centre of a stick of green timber, the water must be expelled.

The only practical mode of removing water rapidly from the cells of wood, is by converting it into vapor; and this requires heat. Water at 212 deg. is converted into steam; because, at this point, the vapor has sufficient elastic force to overcome the pressure of the atmosphere. But if surrounded by air, or any other fluid, at a higher pressure the water would vaporize slowly, and could not escape rapidly.

The condition then to be observed is, that the wood should be surrounded by a medium under less pressure than the elastic force of the escaping vapor, and in proportion to the difference in the pressure, will be the rapidity of escape. This is a very important observation in its bearings on the treatment of timber.

Another observation is also important. Water, at 212 deg., in passing into steam, absorbs a great amount of heat, which becomes latent. The remaining particles are, therefore, rapidly cooled below the point of vaporization. A cubic inch of water, at the boiling point, in its conversion into steam, at the same temperature, absorbs sufficient heat to reduce, if it were possible, $5\frac{4}{10}$ times its weight of the surrounding particles of water, from the boiling to the freezing point.

It is evident, therefore, if a stick of timber could be heated through to a temperature even above 212 deg., and then exposed to the air, the heat rendered latent by evaporation would cool down the remaining water very quickly below the point at which rapid evaporation would be possible. If in a vacuum, or even a partial vacuum, a larger quantity would be removed. But so rapid is the absorption of heat, by a change of form, from a fluid to a vapor, that a long continued application of heat aided by vacuum would be required for the complete expulsion of air and water from the cells.

3. Assuming that a complete, or at least a sufficient, expulsion of air and water has been effected, the third condition of success in the permeation of wood by oleaginous vapors is a temperature sufficiently high to prevent condensation.

The hydrocarbons which distil at certain temperatures are rapidly condensed at lower temperatures; and if vapors distilled at 400 deg. pass into a receiver or tank heated only to 200 deg., there will be an immediate condensation into a liquid, which, falling to the bottom, cannot permeate the wood, which receives, therefore, only the more volatile and least valuable products.

To prevent this condensation, the heat of the tank must be raised nearly or quite to 400 deg.; but if this be done, a new and serious difficulty is presented: the water,

expelled from the wood or condensed from the steam, would be converted into vapor, having a pressure of 18 atmospheres, or 270 lbs. to the sq. in., which would blow a large tank to atoms; and yet, as has been stated, carbolic acid is, of all others, the most important principle contained in coal oil; and carbolic acid boils at 370 deg.; and a much lower temperature in the tank will not maintain it in the form of vapor, or allow it in large quantities to permeate the wood; most of it will be carried off by the tar, and lost. It is probable, also, that a temperature of 400 deg. would make the wood brittle.

No doubt carbolic acid does pass over at lower temperatures, and does, to a slight extent, permeate the wood, being carried over by the mechanical action of vapors at lower boiling points; and it is not impossible that in this way a sufficient quantity may combine with the fermentable elements to protect them effectually. Time, experience, and careful observation can alone determine these questions. But there is one conclusion to which this discussion leads, which is, that there can be no permeation of the fibres of wood in large quantities with carbolic acid, by the vapor process, without using a degree of heat, which, in a large apparatus, would be dangerous and inadmissible, even if experience should prove that the elasticity of the fibres is not destroyed thereby.

On a small scale, and with strong apparatus, wood can no doubt be treated effectually by vapor alone. As the strain per sq. in. upon the iron plates of a tank would be directly as the diameter, a pressure of 200 lbs. per sq. in., in a tank 1 ft. in diameter, would become 1,400 lbs. per sq. in. if the diameter with the same thickness of metal were increased to 7 ft. It is probable that in a very small tank, aided by a bath of oil, satisfactory results can almost always be secured. And there is reason to believe that in this way specimens have been prepared if they have not been, as is most probable, dipped in the oil.

Can there be any introduction of carbolic acid into the cells of wood without employing a dangerous degree of heat?

This is an important question, and it would seem from the principles that have been enunciated, that an affirmative answer can be given.

The first step in any process to fill the pores of wood must be the expulsion of the water from the cells, and it is more difficult to create a vacuum in the cells of dry wood than in those which contain some moisture, since water in its conversion into vapor, expands more than 3,000 time as much as air at the same temperature.

If, when the cells are empty, the tank containing the wood could be filled with liquid oil, and then immediately drawn off, there would be but a small amount of the oil absorbed, for the progress of a dense fluid into the pores would be slow. If, then, steam, air or the vapors from a still, or from any other source, should be admitted, even at ordinary atmospheric pressure, they would penetrate into the empty cells in the interior of the timber, and carry with them a very much larger portion of the liquid carbolic acid and hydrocarbon oils than the vapor process alone could introduce, at the same time the quantity would not be so great as to render the process expensive. The fluid oil also would introduce into the wood the denser and most valuable products that could not be volatilized, giving the wood so treated properties which the vapor process alone could not possibly communicate.

An apparatus that would fulfil all these conditions could be readily constructed, and the expense would actually be less than that now in use, while in safety and efficiency there could be no comparison. In fact, an apparatus was constructed by the writer in 1862, after a series of experiments on flax cotton, for the purpose of impregnating wood with oils, resins, tallow, paraffine, coloring matters, etc., and of dying fibres by the application of heat, vacuum, and pressure, which involves precisely the principles enunciated in this report, and was used in the laboratory of the Treasury Department at Washington.

OTHER PROCESSES FOR PRESERVING WOOD.

James B. Francis, in a recent publication on the preservation of timber, states that 47 patents have been taken out for preserving animal and vegetable substances, including timber. Most of these processes are valueless; those which have commanded most attention will be enumerated and briefly described.

The Bethel Process was patented in

1838, and consists in impregnating timber with the dead oil from the distillation of coal tar, the preparation of which has been described. Joshua Merrill, Esq., of the Downer Kerosene Works, examined this process personally while in England, and his description will be given.

The tank which contains the wood to be treated is of boiler iron. At the bottom and sides are numerous pipes for heating by steam. The timber is placed on an iron car and run into the tank. The tank is filled with dead oil, which is then heated by the steam coils. A pressure of 100 lbs. per sq. in. is applied by means of a hand pump. A thermometer is used to note temperature. The duration of the process is 12 hours. Timber 12 in. sq. is fully impregnated, as is proved by boring holes. An air-pump is also used in connection with the operation, no doubt to remove the escaping air and steam, and relieve the pressure while the wood is being heated in the oil.

This process is conducted on correct theoretical principles; and the results are such as to secure thorough saturation. The timber is heated in oil, the water vaporized, the steam and air which would resist the escape of water from the pores is removed by an air-pump, a vacuum is thus formed, and lastly, the oil is forced in by a pressure of 100 lbs. to the sq. in. applied by a hand pump.

Such a process must be effective, and Mr. Merrill says it is, but the result is saturation. It appears that wood will absorb its own weight of oil, and this is probably at least a hundred times as much as would seem to be required as a preventive of decay. Even in England, where dead oil costs 2 cents per gallon, the process is considered expensive. In this country, where oil is worth 9 cents, the cost of oil per cubic foot would be 33 cents, and including fuel and other expenses the cost of treatment could not be less than 40 cents per cubic foot, if the process should be pushed to the point of saturation.

This is the most effective process for preserving timber known. No case has yet been cited of the decay of a Bethelized stick of timber, or of its being attacked by worms even in the most exposed situations. The heat employed is moderate, and the elasticity of the fibre is probably unimpaired, but the combustibility of the wood is greatly increased, and in a dry climate the use of the process for bridge timber, cars, and railroad purposes generally, would be inadmissible; even paint would not protect such timber from fire, for the exudation of oil causes the paint to scale off.

For these reasons the Bethel process has very properly found no favor in the United States.

Kyan's process was patented in England in 1832, and soon after in this country. "This process, called Kyanizing, consists in immersing the wood in a dilute solution of corrosive sublimate, until it is thoroughly saturated; or if haste is an object, injecting the solution by pressure in a closed vessel, from which the air is first partially exhausted. This process was very extensively adopted in England, and to some extent in this country. It was undoubtedly a valuable invention, and when faithfully executed, it seemed effectually to arrest the rapid decay of timber in exposed situations. The difficulty of carrying on the process, in any way excepting by immersion in open tanks (which is intolerably tedious), arising from the powerful action of the solution upon the common metals, the great cost of the material used, and no doubt, to some extent, the introduction of other processes, has led to the abandonment of Kyanizing in this country. In England, also, it has been given up, where, in addition to the causes operating in this country, the gross frauds practised by the company owning the patent, and by parties carrying on the process, threw a discredit upon it, which with fair dealing it would not have been subject to."

The quantity of solution absorbed and the cost of treatment are the important practical points to be determined.

The absorption of solution would, no doubt be about the same as with the solution of cloride of lime, which Mr. Francis states is 30 imperial gallons of dilute solution containing $\frac{1}{50}$, by weight, of dry salt to 50 cubic ft. of timber. An imperial gallon of it weighs 9 lbs. The solution absorbed would be $5\frac{4}{10}$ lbs. per cubic ft., and the dry salt therein contained, $\frac{1}{10}$ of 1 lb. The cost per lb., in large quantities, is 75 cents, and the value of the salt used per cubic ft. would be $7\frac{1}{2}$ cents, the whole cost of the process possibly, 10 cents.

Margary's process, patented in 1837, consists in immersing the timber in a solution of sulphate of copper. The cost of this salt is 11½ cents per lb., in large quantities, and allowing an equal absorption as with corrosive sublimate, the quantity required per cubic ft. would cost about 12 mills. This process, although extensively used at one time, is said not to have continued in favor.

"*Payne's* process, patented in 1841, consisted in using two solutions in succession, which mutually decompose each other and form an insoluble substance in the pores of the wood. In this manner sulphate of iron and carbonate of soda are said to form an insoluble compound in the pores of the wood, preserves from decay, resists the attacks of insects and renders the wood less combustible. Mr. Francis states that the process has not maintained itself in England, but in France has met with more favor." The cost is not known.

Boucherie's Process.—This process is much used in France for treating telegraph poles and cross-ties, and the traveller along the railways will see at many points elevated tanks from which pipes proceed to rows of sticks laid side by side on platforms, the butts being elevated a foot or two more than the points. The butts are covered with caps, a separate cap to each stick, which fits tightly on the end and is luted with clay or some other material to prevent the escape of the solution. The pressure from a head of 40 ft. or more in the tank, and the downward inclination of the sticks, causes the solution to be forced through the pores and drip from the small end, carrying with it much of the sap and other fermentive matters. This process dispenses with immersion in the fluid, and is simple and inexpensive.

Dr. Boucherie claims that the process accomplishes two objects, that of expelling the sap and of filling the pores with a preservative solution. It is stated that this method has been attended with complete success. The invention seems to be confined to the method of applying the solution and not to the solution itself. The salt used in the process would constitute the principal expense.

Burnettizing Process.—The following description of the Burnettizing process as practised at Lowell is from the report of James B. Francis, Chief Engineer:

"In 1838, a patent was granted in England to Sir William Burnett, for a process for preventing decay in certain vegetable and animal substances, by the use of chloride of zinc. This process called Burnettizing, has been extensively used in England, as a preventive of the decay of timber; and it has been more extensively used in this country, for the same purpose, than any other process. No patent was taken out for the United States. It was first introduced at Lowell, Massachusetts, where, in 1850, the Proprietors of the Locks and Canals on the Merrimack river, at the joint expense of the manufacturing companies, erected the necessary apparatus for carrying on the process. The original intention was to prepare timber only for their own purposes; it was soon found, however, that the apparatus was capable of preparing much more, and that a more extended use would diminish the cost. Accordingly, large quantities of lumber, of various kinds, have been prepared for others, principally for the Lowell Bleachery, and the railroad companies in the vicinity.

"The charges now are—

"For spruce lumber, $5.00 per 1,000 feet, board measure.
"For all other kinds of lumber, $6.00 per 1,000 feet, board measure.
"For spruce shingles, 67 cents per 1,000.
"For other kinds of shingles, 75 cents per 1,000.

"For lots of lumber 100,000 ft., or more, special contracts are made, usually 50 cts. per 1,000 less than the above prices.

"Spruce is prepared at a lower price, as it takes up much less of the chloride of zinc than pine, or other kinds of timber in common use.

"About 1,000,000 ft., of board measure, have been Burnettized annually since 1850 at this establishment. There has been scarcely time yet, to show fully the results of the process here; enough has been seen, however, to show very clearly, that although it is not in all cases a sure preventive of decay, the advantages are more than sufficient to justify its application to most kinds of timber in common use, and in situations favorable to rapid decay. It has also a distinct effect in rendering wood less liable to warp and crack, when placed in dry situations.

"The apparatus at Lowell consists of a

horizontal cast-iron cylinder, in which the timber to be prepared is placed; this cylinder is 60 ft. long, and 5 ft. diameter inside, with one head movable; the iron generally an inch thick. A pair of rails, about 2 ft. gauge, are laid in the bottom of the cylinder, and also on the same line and level, about 75 ft. outside the cylinder; a low truck, about 60 ft. long, runs on these rails. When it is required to charge the cylinder with timber, this truck is drawn out, loaded, the load chained down to prevent its floating, and the truck then drawn into the cylinder.

"A little below the level of the cylinder and parallel to it, is placed a wooden cistern, to hold the solution while the cylinder is being loaded and unloaded, and at times when the apparatus is not in use; this cistern is about 50 ft. long, 7 ft. wide, and 4 ft. deep, and was originally constructed for Kyanizing by immersion. The air-pump is 12 in. in diameter, and 3 ft. stroke. The force-pump 4 in. in diameter, and 2 ft. stroke. The pumps are worked by a small steam-engine of about 15-horse power, which also works the windlass by which the truck is drawn in and out of the cylinder. The boiler of the engine is also used in winter to supply steam to thaw frozen timber; this is done by admitting steam into the cylinder, charged with frozen timber; for several hours before the vacuum is obtained. The steam-engine and boiler are both much larger than necessary for these purposes.

"The chloride of zinc is received from the manufacturers in the form of a concentrated solution, containing about 55 per cent. of the dry chloride. The amount taken up by the wood varies very much, depending upon the kind, dimensions, and dryness of the wood. As the process is conducted at Lowell, it varies from about 10 lbs. to 40 lbs. of the concentrated solution to 1,000 ft, board measure, or say, from 2 to 8 oz. to a cubic foot.

"Unseasoned wood is saturated with great facility and completeness, under the pressure used at Lowell; with seasoned wood it is not so, and although a larger amount of the solution is absorbed, it is frequently found that portions of the interior of the wood have not been reached, even in plank and other stuff of moderate dimensions. Wherever practicable, green and seasoned wood should be differently treated—the latter requiring more time, and, to avoid waste, a weaker solution.

"The most remarkable instances of the preservation of wood, by Burnettizing, are in some of the woods which decay with such rapidity as to be almost valueless. Poplar, for instance, is useless in its natural state, as a fencing material. Trials at Lowell, however, show that, when Burnettized, poplar is a durable wood, apparently as serviceable as chestnut, for posts."

Burnettizing is not as much in favor as formerly. Experience proves that while the process is beneficial to poplar and other soft and perishable woods, it has no effect upon durable kinds, or upon those which contain tannic acid or resins.

Robbins' Process.—The process called the "Robbins" in this country is identical with the Moll process, for which a patent was granted in England in 1835. It consists in the use of the vapors of dead oil. The efficacy of this process depends upon the degree of permeation of the vapors, and the conditions essential to success have been very fully presented and discussed. The numerous testimonials in regard to the efficacy of carbolic acid, the most important constituent of dead oil, would seem to indicate that there can be but little question of its ability to arrest decay and preserve wood indefinitely if actually introduced into the pores; but it has also been shown, in the consideration of this subject, that the vapor bath does not secure this object with any certainty; and of several pieces subjected to the same treatment, in the same tank and at the same time, some may be permeated with vapor and others not at all, according to dimensions, contained moisture, and other conditions of the material operated upon.

Heineman Process.—A company has been formed in New York for the purpose of treating wood with rosin and other substances injected by means of heat and pressure.

No doubt rosin and many other solids, such as wax and paraffine, will preserve wood effectually if the water is first expelled and a vacuum created; but as they do not possess the powerful antiseptic properties of carbolic acid, a much larger quantity of the material will be required, and the cost will be greater.

There are other processes besides Payne's, in which a metallic salt is first

introduced in solution, and other substances are then attempted to be forced into the cells so as to effect a double decomposition, and make deposits of insoluble compounds in the interior of the wood; but an examination of the processes shows that this result cannot be secured by the means employed, and this conclusion is confirmed by chemical tests. The originators of such processes seem to have assumed that because certain chemicals exhibit certain reactions in test tubes and glasses when brought into contact in solution, and form insoluble precipitates, that a similar action must take place in the pores of the wood. The error lies in the fact that the processes employed do not secure that actual contact in the ligneous cells that can be obtained so readily in mixing the contents of glasses.

In one of the processes employed, a saline solution is injected into the cells by vacuum and pressure, and in short pieces, as paving-blocks, there is evidence that this part of the process is successful. The next step is to exhaust the air from the tank containing the wood, then introduce another solution, supposing, that the first will be decomposed and deposits formed in the cells; but in this a great mistake is committed. Air can be removed from the cells by a vacuum, because it is elastic; but water can not except so far as it is driven out by the mechanical action of the escaping air. So long as the cells remain filled with water, not even a thousand pounds pressure to the square inch can force any other solution in to them. There is only one way in which the desired result can be secured: the non-elastic must be converted into an elastic fluid, water must be converted into vapor by the application of heat and vacuum; then the salt contained in the first solution will be deposited in the cells, the cells emptied of water and prepared to receive a second solution capable of being decomposed by the saline deposit of the first.

It is not impossible that valuable results might be secured and timber rendered less combustible and more durable by a properly conducted process of double decomposition, forming insoluble precipitates. Soluble silicates, followed by chloride of calcium, may possibly give satisfactory results; but in all cases applications of heat, and vacuum, will be necessary to remove the water of the first solution, deposit the salt, empty the cells, and prepare for the introduction of the second.

The writer devoted several days to an examination of the portfolios and records of the Patent Office, and finds many processes which approximate very nearly to the conditions essential to success, but they all fall short of attaining this result.

One of the nearest approximations to the conditions of success is found in the specifications of a patent granted to Messrs. Clarke, Hodley & Clifford, of Buffalo. The wood is heated in a *current* of steam to a certain degree, then a fluid bath is used as a condenser, and it is assumed thus the steam expels the air and water.

The current of steam is a new idea, and it may produce more effect than the application of steam not in motion, although no good reason for it can be perceived; but still there can be no movement of the water in the interior cells until it is converted into vapor; the transmission of heat from the exterior is very slow, the steam presses against the outside of the wood with great force, even if there is a current, and retards evaporation, and if allowed to escape more rapidly, so as to relieve the pressure, then the heat is reduced by the expansion. The idea of creating a vacuum in the wood by steam or otherwise, and then following by a bath of oil or other fluid, is just right; but the process described does not accomplish the object, and this is precisely the difficulty with all the patented processes: they do not insure the expulsion of the water.

A. R. McNair, of New York, places wood in a cylinder; heats by steam; creates vacuum by condensation; introduces vapors of coal-oil at a temperature of 392 deg., and follows this by rosin to close the pores. There are many ingenious points in the process, but the radical defect still exists, and needs no further discussion.

The patents on the treatment of wood are very numerous. Only a few more will be briefly enumerated.

W. H. Smith encloses piles with a coating of earthen-ware as a protection against the teredo.

Eben L. Cowling uses steam impreg-

nated with vapor, in which respect his process resembles that of Jos. F. Paul.

Joseph Calkins uses steam to heat the wood, followed by hot vapors and pressure.

George Palmer heats the surface of the wood with fire, and then applies a composition.

George Pustkuchen encloses wood in a cylinder, creates vacuum, and saturates with tar.

Silas Constant and John Smith heat wood in tanks by air, and then expose to vapor of tar.

Voorhees and Custis dry the wood by vapors of oil, then condense, then treat by vapor of oil at 370 deg., the evaporation being carried on in the same chamber which contains the wood, and not in separate retorts.

This list could be continued to a much greater extent, but the illustrations given will probably be considered sufficient.

It is unnecessary to detail all the processes that have been proposed or patents granted for preserving wood from decay. Every conceivable substance, solid, fluid, or in vapor, that could be supposed to have any efficacy, has been suggested. The use of such substances is not patentable, but certain modes of injection and forms of apparatus may be. No patent which covers broadly the use of coal oil or other vapors, or the use of paraffine, wax, or rosin, to preserve wood, can be valid, and yet several patents rest on just such claims.

After completing the experiments in Boston and the examinations in the Patent Office, the writer procured a steam boiler with the use of steam from a factory, and continued experiments in Philadelphia. Pieces of green timber, carefully weighed, were placed in the boiler and heated for half an hour by the direct application of steam; the steam was then condensed so as to allow the water to evaporate from the timber in vacuo. The steam was then turned on again and the process of alternate baths of steam, followed by successive condensations, was repeated for some hours, the condensation being effected in the same cylinder which contained the timber. The result was a total failure; the timber at the end of the process weighed precisely as much as at the beginning. The explanation is obvious, the steam and time restored to the timber the moisture which the preceding application of heat and vacuum had removed.

CONCLUSIONS.

The conclusions from the investigation are :

1. That so long as the cells of wood are occupied by air and moisture, no preservative solutions can be introduced, and the expulsion of air and water must be the first step in any effective process for preserving timber from decay.

2. That water can be expelled by a long continued application of heat, but air only by expansion in a vacuum, and the combination of heat and vacuum will secure the most rapid expansion both of water and air.

3. The preservative fluid must be introduced while the cells are empty, consequently the process must be carried on in vacuo.

4. That no pressure, however great, applied externally to the surface of timber, can force any fluid into the interior so long as air or water is contained in the cells. When air alone is present there may be penetration to a limited extent, superficially, but water is practically incompressible. If, however, the pressure is applied at one end only of a stick, as in the Boucherie process, a fluid may be forced through and exude from the other end.

An apparatus to fulfil the conditions which, from the preceding discussion, appear to be essential to success, must be founded on a process similar to distillation in vacuo. It must consist of at least two vessels, one a receiver corresponding to a retort in which the material can be placed and subjected to the action of heat, the other a condenser in which all escaping vapors can be condensed and the vacuum maintained during the process in both vessels.

The condenser may be of much smaller capacity than the receiver; they should communicate by pipes furnished with stopcocks, and both be supplied with thermometers, vacuum gauges, and pumps.

As an illustration, suppose wood is to be impregnated with dead oil or any other fluid. The receiver must be filled with the wood to be operated on, the door closed air-tight, and the air expelled from both the receiver and condenser.

The expulsion of the air may be effected in various ways.

1. Steam may be admitted at one end to drive out the air at the other end; the subsequent condensation of the steam should leave a vacuum, but in the experiments of the writer, this plan has been only partially successful.

2. The air may be exhausted by an air-pump, but a perfect vacuum cannot in this way be secured.

3. The vessels may be filled with water and the water removed by a pump below the level of the bottom into which the water flows. This should remove all the air excepting that which escapes from the cells.

4. As the atmosphere supports a column of water 33 ft. high, pipes may lead to a tank at a level about 40 ft. lower, where the location is favorable, and thus by filling the vessels with water and opening cocks to allow the water to flow by gravity into the tank, a very perfect vacuum could be produced. This arrangement would be particularly favorable for maintaining a vacuum in the condenser; a pipe in the condenser could throw jets of water in spray from numerous fine perforations, and the water would constantly flow into the tank 40 ft. lower, maintaining a constant vacuum without the aid of pumps. This object can be accomplished in almost any locality by placing the condenser at the top of a building or on trestle work.

Assuming that a vacuum has been created and provision made for maintaining it during the whole process, the next step will consist in the application of heat, which may be done most conveniently by steam-pipes introduced in the receiver. The length of time during which the timber must be subjected to the baking process will depend upon the dimensions of the sticks, and can only be determined by experiment.

It is obvious, however, that the circumstances are favorable to the most rapid evaporation possible; the temperature can be regulated at pleasure, and the removal of pressure by vacuum will give a very low boiling point. As the vapors pass over they will be immediately condensed.

Should the vacuum become vitiated by the escape of air from the cells, it may be improved by the use of an air-pump. The condition of the vacuum will be indicated by the gages.

When sufficient time has been allowed for the wood to dry thoroughly, cocks must be opened connecting the bottom of the receiver with a tank of dead oil, at a lower level. As a vacuum exists in the receiver, the atmospheric pressure will force up the oil and the timber will be immersed in the fluid. When the immersion has continued a sufficient length of time, which also must be determined by careful experiment, cocks may be opened at the top of the receiver to admit air. The oil not absorbed will immediately flow back to the tank from which it was taken, the air pressing upon the exterior of the cells, which are partially filled with oil while a vacuum exists in the interior, will force the oil before it, and thus coat in its progress the interior of the cells. It is probable that in this way a sufficient amount of dead oil may be introduced into the cells to prevent fermentation and decomposition while still far below the point of saturation, and the process may prove rapid and economical.

Instead of admitting air in the manner proposed to expel the oil from the receiver, it is possible that better results may be obtained by allowing the oil to remain until it becomes heated by the steam coils and the vapor collecting at the top expels the oil and penetrates the pores.

Too much oil might be introduced by this mode of treatment, and it is probable that the introduction of air, followed perhaps by a second bath of oil to close the cells superficially and exclude moisture, would give the best results. All these and other questions that may arise can be promptly settled by experiment, and in no other way.

This process of drying in vacuo would be well adapted to the rapid desiccation of fruits, vegetables, fish, meats, etc., with a view to preservation.

The writer does not claim that he has solved the important problem of preserving timber from decay. Before he could satisfy himself or others, a series of continued experiments with suitable apparatus would be required; but it will not be considered egotistical to assume that, in several months of experiment, something has been learned. He is satisfied, at least, that none of the ordinary pro-

cesses will preserve wood economically, and there is, in his opinion, no surer avenue to success in any investigation, than the study of failures and their cause. He has witnessed too many failures to be sure of anything until it has been proven, but believes that in the processes indicated there are strong reasons to expect success.

MOUNTAIN LAKE, Giles Co., Va.

TREATMENT AND UTILIZATION OF SEWAGE.

From "The Engineer."

Probably one of the most difficult tasks with which either a single individual or a body of men can be charged is to collect accurate and reliable information upon any given subject. Sometimes this difficulty may proceed from the fact that there is really very little information of any kind to be obtained, but in by far the greater number of instances it does exist, but cannot be got at. In giving evidence before committees, commissions or other legally authorized tribunals of inquiry, people will volunteer statements by the dozen, which are totally irrelevant to the matter at issue, and, with invincible obstinacy and stupidity, keep back every circumstance which is of vital importance to the inquiry. It cannot be denied that there is a general unwillingness, a sort of dog-in-the-manger tendency manifested by those who possess information, as to imparting it to others. This reluctance would be easy of comprehension if the persons who desired the knowledge intended to apply it solely to their own benefit; but when it is demanded in behalf of the interests and welfare of the whole community, it might be reasonably expected that it would be cheerfully and readily afforded. There is still a third reason, and the one which no doubt is the most active in preventing committees of inquiry arriving at the truth of the particulars they are anxious to verify. It is that while the parties interrogated are really in possession of the information required, and are willing to impart it, they are practically unable to do so. The inability is due to the want of method or system which has distinguished their arrangements. If a company, or local board, be requested to furnish for the benefit of the public, an accurate statement of various operations which may have been conducted by them they are very frequently unable, however willing, to comply with the request. From the loose manner in which many of these companies and authorities carry on their proceedings, from the want of established data, or from the want of a proper organization and efficient superintendence, the collection and abstraction of independent facts becomes well nigh an impossibility. This remark will be found to be especially applicable to the item of cost. Unless estimates are vouched for by strong corroborative evidence, and the accounts are remarkably clear and explanatory, we have long since failed to put any faith in them. The only man who knows what a job costs is the contractor, and that knowledge he very wisely invariably keeps to himself.

If there is one branch of the profession more than another concerning which accurate and reliable facts are of the greatest value, insomuch as they are very few in number, it is that of sanitary engineering. Notwithstanding that there are numerous towns in which various sanitary measures, widely differing from each other, have been either carried out or attempted to be carried out, and of the success or failure of which the local authorities must be well aware, it is nearly impracticable to obtain from them any information on so desirable a subject. From the report of the committee appointed in 1870 to inquire into the "Treatment and Utilization of Sewage," it appears that out of a large number of local authorities who were applied to to assist the committee in their task, by furnishing them with the results of their experience, but very few responded to the request. The committee had a form printed, embodying a series of questions, which they sent to the respective towns, and to which they requested answers, but only eight of them were returned sufficiently filled up to be of any practical utility. It must be admitted that so meagre a return is not very encouraging to those

who are anxious to arrive at an impartial conclusion respecting the advantages of the many sanitary schemes and projects which have from time to time been brought before the public. If, moreover, we subtract from these eight towns from which data have been obtained, Beddington, Norwood and Carlisle, where sewage irrigation has been in progress for eight or ten years, and the results long since recognized as succesful, there are really only five which may be said to furnish corroborative testimony. Again, from these five may be eliminated the town of Leamington, since out of the forty-nine questions contained in the printed form, there are only twelve answered which relate to the subject, none of which are of any importance. So that, summing up, we find that but four towns have supplied any really valuable information, and of these the sewage works of one, Cheltenham, have been in operation only one year; so that it would be somewhat premature to form a definite conclusion in this case, although, judging by analogy, it will no doubt prove another successful example of the utilization of sewage by irrigation. There is the greater reason that this anticipation will be fulfilled, as Cheltenham is naturally well situated for the disposal of its sewage upon this principle, the relative level of the land in the vicinity of the town which is appropriated to this purpose permitting the sewage to be conveyed solely by gravitation. It must be acknowledged that while there is no doubt but that pumping can be employed to advantage with regard to sewage irrigation, and must be used in the majority of instances, yet wherever gravitation will effect the removal and utilization of the sewage, it very materially increases the chance of a pecuniary profit resulting from the undertaking. It is very fortunate for the cause of sewage irrigation that Croydon is so situated that its sewage can be disposed of without the aid of pumping. Had it been otherwise it is questionable whether the irrigation system would have received even the partial development that has attended it.

Granting that very little fresh information on the subject of their inquiry has been obtained by the committee referred to, still several important points have been confirmed thereby. We have invariably, when treating of this question, drawn attention to the necessity of draining the land upon which sewage is utilized. Unless this be thoroughly well effected the sewage will not be utilized to a maximum, and moreover, the effluent water will not reach the requisite degree of purity. There is very little doubt that recent experience has shown that cubical irrigation will supersede superficial, as the former, combined with a thorough under drainage of the soil, possesses superior advantages over the latter with respect to both the sewage and the land itself. It is stated in the report of the committee that if sewage be made to percolate through several feet of soil that has been deeply drained, before it be permitted to flow away, oxidation proceeds as well in winter as in summer, and that whatever amount of nitrogen may be lost, it escapes in an oxidized and inoffensive form. Speaking broadly, it may be said that the utilization of sewage and the purification of the effluent water, are, within certain limits, in proportion to the depth of the soil through which it is caused to percolate. The difference between cubical and superficial irrigation, therefore, is, that in the former the sewage flows through, and in the latter over, the land. Some interesting results were deduced with regard to the effect of these two methods of utilizing sewage upon its temperature. When the sewage was passed throgh the soil the reduction of the temperature was in winter much less than when it flowed over the surface. Again, in summer the former process cools the sewage; while the latter heats it. Roughly, the loss of temperature might be taken as a measure of the degree of percolation and consequently of purification, that the sewage undergoes. By further experiment and confirmation this test might be rendered sufficiently accurate to afford valuable results, more specially as its simplicity and ready application are obvious. A couple of thermometrical observations, one made upon the raw sewage, the other upon the effluent water, would supply all the data necessary. Though we have expressed ourselves as somewhat sceptical respecting the alleged cost of works, yet as the report under notice has given a very elaborate statement of the cost of the Tunbridge Wells sewage farms, it is incum-

bent upon us to make a few remarks thereon. In the interest of all those towns which presumably will have to establish sewage farms, it is only fair to mention that the cost of those at Tunbridge Wells is exceptionally high. The cost of preparing the ground and the necessary carriers, pipes, and fences, comes to as much as £38 per acre. It is admitted that these works have been constructed in a most expensive manner, probably much more so than was absolutely necessary, and it is extremely fortunate that the sewage can be applied on the gravitation principle. If pumping had been requisite, considering that the population is under 20,000, the returns must have been very large to have prevented a heavy burden falling upon the ratepayers.

It has often been alleged by those who are not advocates for sewage farms that cattle fed on sewage-grown grass were liable to suffer from the presence of internal parasitic life. Obviously an allegation of this kind, particularly when made by persons of authority and ability, presents a very serious aspect, and is calculated to create considerable alarm in the minds of the public, besides being exceedingly prejudicial to the cause of irrigation. With the view of setting this matter definitely at rest, the committee sent the carcase of an ox which had been fed for two years on sewage-grown grass, cut from Mr. Hope's farm, near Romford, to be dissected. Dr. Cobbold undertook the operation, and several members of the committee were present on the occasion, and, after a very careful and searching examination, not the last trace of any parasites could be observed. As a proof of the efficient manner in which the operation was conducted, it may be mentioned that several of the muscles were dissected through and through. The result was the same, denoting the complete absence of all entozoa. It must be borne in mind that the animal was fed exclusively upon vegetable products, cut and carried from the land, and did not graze on the farm. It is not, however to be understood from this remark that cattle which graze upon sewage farms are necessarily subject to the presence of parasites, for their germs would in all likelihood pass into the soil and not remain upon the plants. The committee report with respect to Breton's farm, that there is a remarkable absence of those molluscan and insect forms of life which frequently play the part of intermediary bearers to entozoal larvæ. The sewage appears to drive away or kill these creatures. An analysis of the "flaky vegetable tufts" collected from the sides of the carriers, showed that, though they contained numerous active free nematodes, they were perfectly free from the ova of any true entozoon. It is evident that the greater the depth to which the sewage penetrates into the soil the less chance there is of any parasitic germs remaining on the surface. This constitutes another argument in favor of cubical irrigation. Slowly but surely the truth of the irrigation principle is forcing itself upon the minds of the public; doubts that once existed, particularly with regard to its application, are disappearing. Local authorities are beginning to "accept the situation," and to recognize at last that there is no other means of profitably utilizing sewage on a large scale. The various deodorizing and disinfecting schemes are nearly all defunct. It is not, however, to their failure that sewage irrigation will owe anything for its future development, as even during their imaginary success they never proved anything against the principle.

COMPRESSED AIR ENGINES.

From "Engineering."

The plan of working tramway cars or other vehicles by turning to account the power stored up in a supply of compressed air, is one which has been frequently proposed, and which has, in several instances, been carried out in practice with results in some cases satisfactory, and in others of a decidedly opposite kind. In America, in particular, this system of propulsion has attracted much attention, and we, from time to time, receive from the United States reports of the more or less success-

ful trials of various modes of carrying out the system in practice. It unfortunately happens, however, that the data available concerning these trials are generally of the vaguest possible kind. They inform us, perhaps, that a car fitted up with a compressed air engine made such and such trip in such and such time, and that the performance was successful, or that it failed in some way or other; but we are doubtful if any really complete series of experiments has ever been made with one of these engines, or at all events we are unaware of a report of any such experiments having been published. By a complete series of experiments on a compressed air engine we mean a trial, or series of trials, of the engine on the line of which the gradients are known, and with a load also known, there being taken during the run or runs, a continuous series of indicator diagrams from the cylinders, and contemporaneous observations of the temperatures and pressures of the air in the reservoir, and in the pipe leading to the cylinder, besides a record of the temperature of the air as it is discharged from the cylinder into the atmosphere. The barometric pressure and the temperature of the external air, and the amount of moisture present should also be noted, while a record might also be kept of the temperatures of the external surfaces of the reservoir, of the pipes communicating with the cylinders, and of the cylinders themselves.

From such a series of observations as that to which we have alluded, some valuable data might be deduced bearing upon the construction of pneumatic locomotives, and we believe that a great deal of the time and money at preset spent in so-called practical experiments might be saved. It would be possible, for example, to deduce from such observations, information respecting the rates at which the air during expansion took up heat from objects with which it was in contact, and to determine the extent to which the heat converted into work was replaced by other heat absorbed by external sources. These data being obtained, it would become possible to determine approximately the best proportions between the initial and working pressures of the air used (for in most cases the pressure of the air is materially reduced on its way from the reservoir to the cylinders), the best cylinder capacity to employ for a given amount of work to be performed, and the best place to make up—or try to make up—the loss of heat due to work done. The determination of these points, even approximately, would be of immense service to those who are endeavoring to bring pneumatic propulsion into use, and instead of working in the dark, as at present, most—if not all—of them appear to be doing, we should earnestly recommend them to carry out such trials as those of which we have spoken. If they were to do so we feel certain that the cause in the progress of which they are interested would be most materially benefited.

From the time that the system of working engines by compressed air was first proposed, down to the present, one of the most annoying practical difficulties with which the advocates of the system have had to struggle, has been the excessive refrigeration caused by the expansion of the air, and the consequent freezing of the moisture contained in it; this freezing resulting in the blocking up of the pipes and passages with ice. To avoid this trouble, it has been proposed to heat the air on its way to the cylinders by passing it through pipes enclosed in a stove or furnace, or to envelop the cylinders in jackets through which the products of combustion of a stove might be made to circulate; while, in the case of the submarine boats built at St. Petersburgh some time ago for the Russian Government, and which are driven by engines worked by compressed air, the plan was resorted to, of connecting the engines with the reservoirs by a number of pipes, and shutting off the current of air through any of those pipes in which ice might accumulate, so that the latter might be thawed by the absorption of heat from external sources. In some cases upwards of thirty pipes were used to connect the engine with the reservoir of air. This plan of employing a number of connecting-pipes no doubt meets a portion of the difficulty; but it is a plan requiring more attention than it is desirable should have to be bestowed on the working of engines fitted to tramway cars, while the proposals to heat the air on its way to the cylinders, by passing it through tubes exposed to the fire of a stove or furnace, or to heat the cylinders by means of jackets traversed by products of combustion, are also open to ob-

jection on practical grounds. Amongst other things there is, if either of these latter plans be resorted to, always a risk of giving to the air, prior to its admission, or to the cylinder itself, such a temperature as to destroy the lubricating material employed, and thus cause "cutting" to take place.

Having objected to the principal plans proposed for obviating the difficulties resulting from the refrigeration of the air during expansion, it is only fair that we should point out what we believe to be a preferable mode of getting over these difficulties. This mode consists simply in the employment of a hot-water jacket for both the cylinders and reservoir, and in the adoption of a sufficiently slow piston speed. The water jacket may consist simply of a tank well lagged externally, and sufficiently large to contain the air reservoir, the connecting pipes, and the cylinders; or, if preferred, the reservoir and the cylinders may be enclosed in independent jackets or tanks, it being merely necessary to take care that each jacket contains a sufficient supply of hot water to furnish the heat required of it during the run. It may, at first thought, be supposed that the quantity of water required to supply this heat would be so great as to render the plan proposed an impracticable one. So far from this being the case, however, the quantity required is remarkably small, as we shall show on a future occasion, when we treat of the question in greater detail, and we may remark here that for running a 6-ton car for 3 miles over a fair ordinary road, the quantity of hot water required for the jackets would be but about 20 gallons only, it being supposed that this quantity is renewed by a fresh supply at a temperature of 200 deg. or reheated, say, by blowing steam into it, at the commencement of each trip. In some cases where a stove is employed to warm the car, the heat from this stove could be turned to account for maintaining the temperature of the jacket water.

We have mentioned that, in addition to using hot-water jackets, it appears to us desirable that a slow piston speed should be adopted for compressed air engines, and we may explain briefly why we hold this opinion, an opinion which may, perhaps, be opposed to the experience of some who have worked such engines without jacketed cylinders. Let us, for instance, consider the case of an engine in which the air, stored up at a high pressure in a main reservoir, is led thence, through a reducing valve, to an intermediate receiver, in which it is maintained at a constant pressure, while from this receiver it passes to the cylinder, and is cut off in the latter at, say, one-third the stroke. In such an arrangement the work expended in the intermediate receiver in supplying air to the cylinder is balanced by that developed by the entrance of the air from the main reservoir, and under these circumstances the intermediate receiver is exposed to neither a loss nor gain of heat, so long as the air flowing into it from the main reservoir is maintained at a constant temperature. The work done during the first third of each stroke prior to the valve closing the admission port, is really done by the expansion of the air in the main reservoir, and it is to that reservoir, consequently, that the heat must be supplied if the air flowing to the cylinder is to be maintained at a constant temperature. With a hot-water jacket there need be no difficulty in adopting such a form of main reservoir as will enable this required amount of heat to be communicated to the contained air with certainty. After the point of cut-off is passed, however, the work done in the cylinder during each stroke is performed by the expansion of the air contained in the cylinder, and this expansion is, of course, accompanied by a refrigerating effect. To prevent the temperature of the air in the cylinder falling too low during this expansion, it is desirable to supply heat from external sources while that expansion is taking place. But air takes up heat but slowly from the surfaces with which it is in contact, and hence, that the heating to which we have referred may be sucessfully performed, it is necessary either to envelop the cylinder in a jacket of which the contents are kept at a high temperature, or to employ such a slow piston speed that time is given for the absorption of heat from a jacket of which the contents are but moderately heated. For several practical reasons we prefer the latter alternative, and hence it is that we advocate slow piston speeds for compressed air engines. What the best piston speed may be it is impossible at present to say, as data for

arguing the question properly are wanting; but we consider that there is evidence available to show that this speed is much lower than has hitherto been generally adopted for such engines, and we believe that the matter is one which has not hitherto received the attention which its importance deserves. Air, it must be remembered, possesses very different qualities to steam, and the arguments in favor of high-piston speeds for steam engines certainly do not appply to motors worked by compressed air.

We have now directed attention to some of the leading points connected with the system of working engines by compressed air; but we have very far from exhausted the subject. There are said to be more ways of killing a dog than hanging him, and it is equally true that there are more ways of getting rid of the trouble arising from frozen moisture in the class of engines of which we have been speaking than that of jacketing the reservoir and cylinders. A consideration of these methods, however, we must leave for a future article, when we shall also have something to say of the system from a quantitative point of view; or, in other words, shall speak of the amount of compressed air which it is necessary to carry to perform a given amount of work, and shall probably treat of the efficiency of the system as a whole.

SOLAR HEAT.

By Captain JOHN ERICSSON.

From "Nature."

The calculations presented by Père Secchi, in his work "Le Soleil," relative to solar temperature and solar radiation, tending to discredit the result of recent investigations on the subject, I have carefully examined the "solar intensity apparatus," the indications of which form the basis of those calculations. This unique device will be found delineated on p. 267 of the work referred to, the accompanying illustration (Fig. 1) being a facsimile of the same. It represents a longitudinal section through the centre line, thus described:—A B and C D are two concentric cylinders soldered one to the other; they form a kind of boiler, the annular space being filled with water or oil at any temperature. A thermometer, *t*, passes through a tube, across the annular space, to the axis of the cylinder; it receives the solar rays introduced through a diaphragm, *m n*, the opening, *o*, of which is very little larger than the bulb of the thermometer. A thick glass, V, closes the back part of the instrument, and admits of ascertaining whether the thermometer is placed in a direct line with the pencil of rays. The interior cylinder and the thermometer *t* are coated with lamp black. A second thermometer, *t'*, shows the temperature of the annular space, and consequently that of the inclosure. The whole apparatus is mounted on a support having a parallactic movement, to facilitate following the diurnal motion of the sun. The apparatus being exposed to the sun, it will be found, on observing the two thermometers, that their difference of temperature increases gradually, and that in a short time it ends by being constant.

Before pointing out the peculiarities of the contrivance thus described by Père Secchi, it will be instructive to examine his "solar intensity apparatus," manufactured by Casella, represented in Fig. 2. The manufacturer publishes the following statement regarding this instrument:—"Two thermometers are here kept immersed in a fluid at any temperature, and a third surrounded by the same conditions, but not immersed, is exposed to the rays of the sun. The increase of temperature thus obtained is found to be the same, irrespective of the temperature of the fluid which surrounds it." No one acquainted with the principles which govern the transmission of heat within circulating fluids can fail to observe that the thermometers applied above the central tube will not furnish a reliable indication of the temperature of the fluid below the same, nor of any portion of the contents of the annular space towards the bottom. Apart from this defect, it will be perceived that an upward current of atmospheric air will sweep the underside of the external cylinder, causing a reduction of tem-

perature of the fluid confined in the lower half of the annular space. Again, the heat radiated by the bulb of the thermometer exposed to the sun will elevate the temperature of the air within the central tube, and consequently produce an internal circulation tending to heat the upper part of the fluid contained in the annular space. The effect of the irregular heating and cooling thus adverted to will be considered after an examination of the result of some observations recorded in Table A, conducted at different times during the month of September 1871. In order to insure an accurate position, the instrument during these observations was mounted in a revolving observatory upon a table turning on declination axes provided with appropriate mechanism and declination circle. An actinometer being attached to the same table, the true intensity of the radiant heat, as well as the sun's zenith distance, were recorded simultaneously with the indications of the Secchi instrument furnished by Casella. Let us first consider the tabulated observations of September 2 recorded at equal intervals of three minutes. The indication of the two thermometers immersed in the fluid contained in the annular space first claims our attention, since the temperature of this fluid is the principal element in Père Secchi's computations of solar temperature. It will be seen on referring to the second and third columns of the table, that, while the upper thermometer indicates a mean temperature of 86.9 deg., the lower one shows only 79.5 deg., difference = 7.4 deg. This great discrepancy of temperature at different points of the upper portions of the annular space at which, owing to the inclined position of the concentric tubes, something like uniformity ought to exist, suggests a still greater discrepancy of temperature at the underside towards the lower termination of the tubes. In addition therefore to the observed irregularity of temperature at the upper part, shown by the table, no indication whatever is furnished of the temperature of the fluid in the annular space below the central tube, nor towards the termination at either side. Obviously, then, no accurate computation can be made of the degree of refrigeration to which the central thermometer is exposed by the radiation from the cold blackened surface of the internal tube, every part of which, as we have seen, possesses a different temperature compared with the rest, consequently transmitting radiant energy of different intensity. It will be found practically impossible, therefore, to determine the true differential temperature of the contents of the bulb exposed to the sun's rays and the fluid contained in the annular space. Hence, the differential temperature entered in the table, the result of comparing the indications of the thermometers, is manifestly incorrect. It will be found also by reference to the table that while the mean temperature imparted to the central thermometer by the sun's rays is 93.1 deg., the mean temperature of the fluid in the annular space is 83.3 deg. Consequently, the intensity of solar radiation established by the instrument is only 93.1 deg.—83.3 deg.= 9.79 deg Fahr. Now, the sun during the recorded experiment of September 2 was exceptionally clear, the mean indication of the actinometer while the experiment lasted being 60.05 deg., thus showing that the energy developed was only $\frac{9.79}{60.05} = 0.16$ of the true radiant intensity. The mean zenith distance, it may be mentioned, was only 33 deg. 24 min. during the experiment. Agreeable to the table of temperatures previously published, the maximum solar intensity for the stated zenith distance is 63.35 deg.; thus we find that the sun, as stated, was exceptionally clear while the trial took place, which resulted in developing the trifling intensity of 9.79 deg. Fahr. The result of the experiments conducted September 6th, recorded in the table, it will be seen was nearly the same as that just related, the mean temperature indicated by the thermometer exposed to the sun being 98.2 deg., while the mean of the two thermometers immersed in the fluid was 87.8 deg., hence the differential temperature 98.2 deg.—87.8 deg.=10.4 deg. The mean temperature of solar radiation during the experiment, ascertained by the actinometer, was 59.75, the zenith distance being 35 deg. 33 min. Consequently, the intensity indicated September 6th was only $\frac{10.45}{59.75} = 0.17$ of the true energy of the sun's radiant heat, against 0.16 during the previous experiment. It will be observed that the fluctuation of

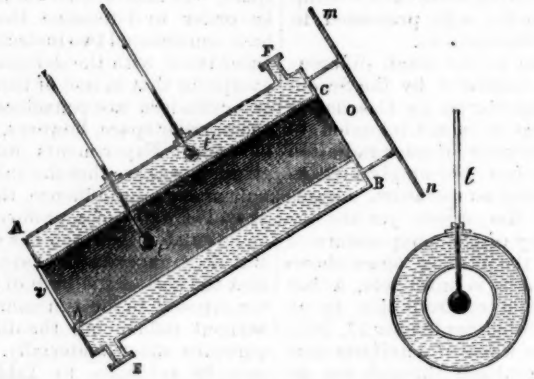
the differential temperature was much greater September 2d than during the succeeding experiment, owing, no doubt, to the influence of currents of air produced by a strong breeze on the first occasion, the revolving observatory being partially open on the side presented to the sun during observations.

With reference to the small differential temperature indicated by the Secchi instrument manufactured by Casella, it may be urged that it is not intended to show the true intensity of solar radiation on the earth's surface, but simply a means of determining solar temperature. Granted that such is the object, yet the extreme irregularity of the temperature of the fluid within the annular space shows that the instrument is unreliable, a fact established beyond contradiction by an experiment instituted September 27, 1871. On this occasion water of a uniform temperature was circulated through the annular space. This was effected by gradually charging this space from the top, and carrying off the waste at the bottom, holes having been drilled in the external casing for that purpose. The result of this conclusive experiment is recorded at the foot of Table A. It will be found on reference to the figures, that the mean difference of the two thermometers immersed in the fluid was only 64.9°-64.4= 0 5°, while the mean differential temperature was augmented to 79.1°-64.45=14 .65° against 9.79° on the 2d of September, although the zenith distance was greater, and the solar intensity less; circumstances which ought to have *diminished* the indicated intensity. It is needless to enter into any further discussion of the demerits of the instrument represented in Fig. 2. We may now return to the consideration of the device delineated in Fig. 1, copied from "Le Soleil." It will be seen that the material difference of construction is that of applying only one thermometer for ascertaining the temperature of the fluid in the annular space Possibly this single thermometer may indicate approximately the mean temperature of the upper and lower portions of the fluid above the central tube; but it furnishes no indication of the temperature below, nor at either extremity of the annular space. The inadequacy of the means adopted for ascertaining the temperature of the internal surface which radiates towards the bulb of the central thermometer having thus been pointed out, it will be well to consider whether the expedient of passing a stream of water of nearly uniform temperature through the annular space, will insure trustworthy indication. In order to determine this question, I have constructed two instruments, in strict accordance with the delineation in Fig. 1, excepting that in one of these the concentric cylinders are considerably enlarged; the annular space, however, remaining unchanged. Experiments with the two instruments prove that the enlargement does not materially influence the indications, provided water of a uniform temperature be circulated through the annular space. But these experiments have demonstrated that the size of the bulb of the thermometer exposed to the sun cannot be changed without influencing the differential temperature most materially. This will be seen by reference to Table B, which records the result of experiments with different thermometers, and tubes of different diameter, conducted October 17, 1871. As on previous occasions, the instruments, in order to insure accurate position, were attached to the declination table arranged within the revolving observatory. The bulbs of the thermometers employed were very nearly spherical, their diameters being respectively 0.30 and 0.58 in. The upper division of Table B, which records the experiment with the *small* bulb exposed to the sun, establishes, it will be seen, a differential temperature of 14.4 deg. for the instrument having the 1¼-in. central tube, and 16 deg. for the one having the 3-in. central tube. Referring to the lower division of the same table, it will be seen that when the thermometer with the *large* bulb is exposed to the sun, the differential temperature reaches 22.5 deg. in the instrument containing the 1¼-in. central tube, and 21.1 deg. in the one having the 3-in. tube. We thus find that, by doubling the diameter of the bulb of the thermometer exposed to the sun, all other things remaining unchanged, an augmentation of the differential temperature amounting to nearly one-third takes place. This fact proves the existence of inherent defects fatal to the device delineated in Fig. 1, rendering the same wholly unreliable.

Agreeably to the doctrine of exchanges, the diameter of the bulb is an element of no moment, since the internal radiation

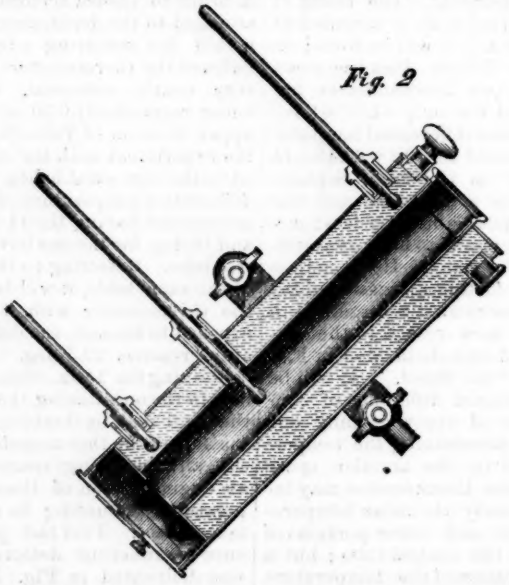
towards the same—*provided its temperature be uniform*—depends solely on the temperature and annular distances of the radiating points of the enclosure. Infallibility of the "solar intensity apparatus" has evidently been taken for granted on the strength of the soundness of this doctrine, as we find no allusion to the size of

FIG. 1.



the bulb in M. Soret's account of his observations of solar intensity on Mont Blanc; nor does Mr. Waterston, who employed a similar instrument during his observations in India, advert to the dimensions of the bulb of the thermometer exposed to the sun. These physicists apparently overlook the fact that, while the entire convex

Fig. 2



area of the bulb is exposed to what may be considered the cold radiation from the enclosure, only one half receives radiant heat from the sun. This circumstance would be unimportant if the heat thus received were instantly transmitted to every part; but the bulb and its contents are slow conductors, while the conducting power di-

minishes nearly in the inverse ratio of the square of the depth. Consequently, by increasing the diameter, the parts of the bulb opposite to the sun will receive considerably less heat in a given time than if the diameter be diminished.

TABLE A, *showing the result of observations made with Secchi's "Solar Intensity Apparatus," manufactured by Casella.*

SEPTEMBER 2, 1871.

Thermometer exposed to the Sun.	External Casing.			Differential Temperature.	Zenith distance.
	Upper Thermometer.	Lower Thermometer.	Mean.		
Fahr.	Fahr.	Fahr.	Fahr.	Fahr.	° ′
83 5	76 0	70 0	73 0	10 5	33 0
84.2	77 0	71.5	74 2	10.0	
85 5	79 0	74 2	76.6	8.8	32 50
86 0	83 5	74.5	79.0	7 0	
89.0	84 0	75.5	79 7	9 2	33 0
90.5	85.0	76.5	80.7	9.2	
92.0	85.5	78.0	81.7	10 2	33 10
93.0	86.5	79.0	82 7	10 2	
94.0	87.8	80.0	83 9	10 1	33 21
94.5	89.0	81.5	85.2	9 2	
95.5	90 0	82 5	86 2	9 2	33 32
96.5	90.5	83.5	87.0	9 5	
98 0	91.5	84.5	88 0	10.0	33 44
99 0	92 0	85.0	88.5	10 5	
100.0	93.0	86.0	89 5	10.5	33 56
101.0	93.5	86.5	90.0	11 0	
101.5	94.0	87.0	90.5	11 0	34 8
93.1	86.9	79.5	83.3	9 79	33 24

SEPTEMBER 6, 1871.

94.5	88 0	81.5	84.7	9.7	35 56
95.5	88 5	83 0	85 7	9.7	
96 5	89 5	84.5	87 0	9.5	35 41
97.5	90 0	85 0	87 5	10.0	
98.0	90.0	85 0	87.5	10 5	35 26
98 5	90.5	85 5	88.0	10.5	
99 0	90 5	85.7	88.1	10.9	35 11
100.0	91.0	86 5	88.7	11.2	
100.3	91.0	87 0	89.0	11 3	34 56
100.3	91.2	87 5	89.3	11.0	
100.5	91.5	88.0	89 7	10.8	34 41
98 2	90.2	85 3	87.8	10.45	35 33

SEPTEMBER 27, 1871.

78.5	64 0	64 0	64.0	14.5	44 0
79 0	65.0	64.0	64 5	14 5	
79.5	65.0	64.5	64.7	14 7	44 55
79.5	63 0	65.0	64.0	15.5	
79 5	64 0	65.0	64 5	15.0	45 51
79.0	64.5	65.0	64.7	14.2	
79.0	64.5	65 5	65 0	14 0	46 48
79 0	64 5	65.5	65 0	14 0	
79.0	65.0	65.5	65 2	13.8	47 46
79.1	64.4	64.9	64.65	14.45	45 16

TABLE B, *showing the result of employing different thermometers.*

Diameter of bulb 0.30 in.

1¼-inch tube.			Zenith distance.	3-inch tube.			Zenith distance.
Sun.	Fluid.	Diff.		Sun.	Fluid.	Diff.	
Fahr.	Fahr.	Fahr.	° ′	Fahr	Fahr	Fahr.	° ′
74	60	14	50 32	77.5	62 1	15.4	49 54
74.5	60.3	14.2	50 24	78.5	62.3	16.2	50 3
75	60.7	14 3	50 16	79	62.5	16 5	50 12
75 5	61	14.4	50 8	79	63	16	50 21
76	61	15	50 1	79	63	16	50 30
75.0	60.6	14.4	50.16	78.6	62.6	16.0	50 12

Diameter of bulb 0.58 in.

1¼-inch tube.			Zenith distance.	3-inch tube.			Zenith distance.
Sun.	Fluid.	Diff.		Sun.	Fluid.	Diff.	
Fahr.	Fahr.	Fahr.	° ′	Fahr.	Fahr.	Fahr.	° ′
83.6	62.6	21	49 54	79 2	60.1	19 1	50 32
85.5	63	22.5	50 3	81	60.3	20.7	50 24
86.4	63.4	23	50 12	82 5	60.7	21.8	50 16
86.7	63.5	23 2	50 21	82.7	60.7	22	50 8
87.7	63.7	23	50 30	83	61	22	50 1
85.9	63.2	22.5	50 12	81.7	60.6	21.1	50 16

CARBONIZED SEWAGE.

From "Engineering."

All processes for the manufacture of sewage into poudrette may be classed under the above heading, although in some cases the process of carbonization is not carried so far as in others, and in only one of all these is any attempt made to utilize the gases thrown off from the sewage matter during the process of carbonization. This is known as "Hickey's system of conservancy by carbonization." Mr. Hickey is engaged in India, where the Government has so far taken up the question as to grant certain small sums for testing the efficacy of that gentleman's invention. The main features of Mr. Hickey's plan of utilizing sewage are that he collects the gases evolved during carbonization, which he proposes to make available for town illumination, whilst the poudrette or coke that remains has been found to be a most excellent deodorizer, and, mixed with the ammoniacal liquors collected from the gas retorts, it also forms a valuable manure. Such are the claims put forward by Mr. Hickey in favor of his invention, and, so far as it has hitherto been tested, the results would appear partially to justify the expectation that some considerable advantages may be expected to arise from its introduction. Whether it will ever be found practicable to illuminate our towns with gas manufactured from the contents of their respective sewers, is, we need scarcely say, very doubtful. The system of water-carriage for sewage is altogether opposed to the success of the carbonization process, in so far as its application for the manufacture of gas is concerned, in consequence of the large amount of fuel that would be required to drive off the water by evaporation before

the other contents of the retort would begin to give out gas in any quantity. The destruction of sewage by fire is probably one of the oldest methods of disposing of it, and has been used in India.

The production of burning gas for lighting purposes, by the carbonization of ordure, is not, as is generally believed, a novelty, for so far back as 1686, a certain savant named Dalsevius made experiments in Paris, and satisfactorily proved that an inflammable gas could be obtained by exposing organic matter to a very high temperature in hermetically closed vessels, and further, that an attempt was made at Paris in 1841 by Boussingault and Payen to prepare, by the carbonization of night refuse, an inodorous manure, and so far succeeded, that it was classed amongst the best of its kind, as in 1,000 parts, there were 29.6 of nitrogen, against 12.4 in ordure in its natural state, and this fertilizing constituent obtained for it a very remunerative commercial value. Since then, the carbonization of fecal matter has continued to be practised in France by Mr. Salmon, a distiguished chemist, who has rendered great service to hygiene by his discoveries, and the application of chemistry to the disposal of the carcases of dead animals and other organic matters in such a manner as to render them quite innocuous, and at the same time useful for industrial or agricultural purposes. Mr. Salmon's method of manufacturing poudrette or artifical guano, is to mix up the night-soil with a certain quantity of finely powdered charcoal, poudrette, or noir animalisé (such is the name given to the residue of the carbonization of night refuse), to deodorize the large quantities of fecal matter collected daily in his grounds. The mixture is afterwards put into hermetically closed iron cylinders and carbonized, and the gas escaping from the cylinders is turned to account by being introduced into the furnace; and the burning of the gas, it is said, considerably reduces the consumption of fuel.

In order to utilize the gases given off by heat, as well as to preserve the charred residuum, which is thus proved to be effective not only as a deodorizer, but it is also valuable commercially for agricultural purposes, special appliances are required, the provision of which, as well as the expense of fuel, establishment, etc., must be taken into consideration in estimating the financial results of this method of disposing of town sewage. The desire to make the disposal of sewage absolutely remunerative to the community is, we think, a mistake, for whatever means may be adopted with that view, the one great end undoubtedly is to remove what would otherwise become a nuisance, and it is therefore a convenience which may justly be expected to have to be paid for. But whilst that is the case, the sewage itself has undoubtedly a commercial value, and it should not therefore be allowed to run to waste, but be converted, by whatever process may appear must suitable under the circumstances of each case, provided the process of such conversion does not greatly exceed in cost the price at which the product realized can be sold for.

There are many who are opposed, on sanitary grounds, to water-carriage for sewage in tropical countries, owing to the more rapid decomposition and consequent emanation of sewer gases which takes place in hot climates than where the temperature is more moderate. This question is now undergoing a practical test in several of the largest cities in India, where a regular system of sewers and water supply renders the adoption of water-carriage practicable. At the same time other methods of disposing of town sewage are being tested. The dry earth system of conservancy has been in use in the Madras Presidency since the latter part of 1863, and it has also been adopted in other parts of India in barracks, hospitals, and prisons. Mr. Hickey's plan of conservancy by carbonization is a more modern introduction, and can hardly at present be considered to have advanced beyond the experimental stage; but so far as it has been already tested, it would appear to offer advantages to a country like India, superior to that of water-carriage in every respect excepting one, and that is the conveyance of all the sewage of a town to one place of outfall. Experiments recently made in India on a small scale, where the excreta of sixteen men for one day was treated in a small experimental gas-making apparatus, proved that the proposed process of converting such matters into gas and coke was perfectly practicable, and

easy. In estimating the cost of the process from the results obtained by this experiment, it was calculated that the cost of coal for evaporation of the liquid portion of the sewage, and the subsequent carbonization of the solids, would be at the rate of four rupees per thousand of population per day, in addition to which must be added the expense of first cost and wear and tear of machinery, as well as the cost of labor required in the manufacture. The total amount of gas per thousand of population, raised to a temperature of 870 deg. Fahrenheit, was equal to 1156 cubic ft., which when made to burn while issuing under pressure from a large orifice, gave a flame which appeared to diffuse a considerable amount of light; but when tested by an ordinary burner, the light was pale and feeble. When tested in a photometer its illuminating power was found to be only equal to 2½ candles. Tested against the ordinary gas burnt in Calcutta, its value was only one rupee four annas per thousand cubic ft., representing a total value of one rupee seven annas (three shillings nearly) per thousand of population. The amount of charcoal remaining in the retort per thousand, weighed, when quite dry, 39 lbs., valued at about one shilling. In addition to the above, there is also nearly 2 lbs. by weight of ammonia yielded by the same quantity of refuse. The charcoal contains more than half its weight of ash, and is therefore useless as a heating agent. According to the above calculation the value of the products would appear to be about four shillings per thousand head of population per diem, and the cost of producing them eight shillings for coals alone. An analysis of the gas produced shows it to consist of the following:

Hydrogen	35.36
Marsh gas	31.28
Olefiant gas and other heavy hydrocarbons	2.40
Carbonic oxide	21.74
Carbonic acid	9.00
Phosphoretted hydrogen, a trace, say	0.22
	100.00

The poudrette remaining after making the gas, appears to be better suited than ordinary earth for carrying out Moule's system of dry conservancy; for whilst dry earth has the power of absorbing from 30 to 35 per cent. of moisture, the poudrette absorbs, under similar circumstances, 100 per cent., so that while 8 lbs. per head per diem of the former are required to carry out Moule's system, only about 2½ lbs. per head per diem would be required of the poudrette to carry out a modification of Moule's system, which, whilst being equally efficacious, would be much less expensive. Any amount of poudrette in excess of what might be required for the above purpose would, when mixed with the ammonia obtained during this process, form a most valuable manure.

THE EMERALD MINES OF MUZO.

From "Journal of the Society of Arts."

The past history and present condition of the emerald mines of Muzo, which are situated in the State of Boyaca, four easy days' journey from the capital, derive considerable interest from the fact that, with the exception of the East Indies, the emerald can only be found at Muzo. Mr. Bunch, secretary of legation at Bogota, has obtained most of the information contained in his notice of the subject from his French colleagues, who would naturally be in communication with the French company now in possession of the mines. According to a law recently passed by the Columbian Congress, the present strict monopoly, under which these mines are held by the Government, will cease in 1874, at the expiration of existing agreements, when it will be free for any one to work them.

From certain indications, apparent to persons residing upon the spot and acquainted with the country, it is evident that the mines of Muzo were known long before the discovery of America and the conquest of Granada by the Spaniards. When an expedition came in the year 1553, under the orders of Don Juan de Lancheros, to reduce the tribe called "Los Muzos" to the Spanish rule, these Indians were found to possess a large quantity of emeralds. How they worked the mines it is not easy to understand, as they had no tools of iron. It is sup-

posed that they had found the stones in the bed of the mountain torrent, for it sometimes happens that the winter rains produce great land slides, which lay bare large veins of emeralds, the stones in which are washed out by the waters. But these gems are of a very inferior quality; they resemble those which are still found in the Indian burial places, or in the lakes into which the Indians used to throw their riches during their contests with the Spaniards. However this may have been, the mines of Muzo were worked soon after the arrival of the Spaniards, on a large scale, both in the open air and by means of subterranean galleries; but about the middle of the eighteenth century the labors were abandoned, no one knows why. The old people of the district relate that their fathers used to work at the mines until the mountains vomited flames, which lasted for many years, and made it impossible to go on. This tradition resembles a fable, especially as there are no signs of a volcanic eruption near Muzo. It is, therefore, impossible to say why the labors were interrupted. Not until after the War of Independence and the expulsion of the Spaniards, were they resumed, when the Republic took possession of the mines, and let them out to individuals and to companies.

About the year 1844, a Columbian, named Paris, carried a large quantity of emeralds to Europe and the United States, many of which he sold for considerable prices. In order to disarm hostility at home, he presented to the city of Bogota various valuable objects, such as a fine bronze statue of Bolivar, cast by Tenerani, of Rome, a clock for the cathedral, instruments for the observatory, etc. He died a rich man. In 1864, a French company obtained a grant of the mines for ten years, for an annual payment of $14,700 (about £3,000), together with a monopoly of all emeralds in Columbia, the Government binding itself to prohibit the working of any other mines which might exist in the territory of the Union. This company is now in possession of Muzo, where the works are directed by a French engineer, named Lehmann. In the opinion of this gentleman, the mountains of Muzo are rich in emeralds, the quantity hitherto extracted being almost inappreciable. The principal mine now worked is pierced in every direction by galleries made by the Spaniards. Since 1825 it has been worked in the open air; an immense number of gems have been found, many of them of great value. After this mine shall have been exhausted, which will not be for many years, not a thousandth part of the ground containing emeralds will have been touched. This consists of a chain of mountains which extends beyond human sight. At two days' journey from Muzo, there is another mine, called "Lasquez," which was just touched by the Spaniards, and is evidently very rich. All this ground, including Lasquez, bears traces of the presence of the Spaniards; and, as the geological formation is the same in the whole neighborhood, it is clear that the day is very far distant when these mountains shall be exhausted; in fact, they may be said to be inexhaustible.

The mountains of Muzo, like those of the whole central Cordillera of the Andes, belong to the lower formation of chalk. If the structure be observed of one of the portions now worked, the emeralds will be found in two distinct layers,—the first, or upper one composed of a calcareous bitumen, which is black and friable; the second, lower down, also of calcareous bitumen, but hard and compact. These two layers are generally separated from each other by a distance of from seventeen to twenty-two yards. In the upper layer are found the veins which yield the "nests" of emeralds, that is to say, a number of these gems massed together. But after one of these nests the vein disappears, being crossed by others of a different kind, which run in a different direction to those containing the emeralds. These latter veins are called "cenicerous," from their ash-color. They are generally horizontal, whilst the emerald veins are perpendicular; they all run from north-east to south-west. The veins of the lower layer are more regular, and are followed for fifty or sixty yards, and even more. "Nests" of emeralds are very seldom found in them, but they are more easy of extraction. When veins of fluor-spar, well crystallized, are met with, the emerald is not far off; the presence of rock crystal is also a good sign, as likewise that of a pretty pyramidal shape, of the color of honey, called "parisite."

The mine of Muzo is worked both by

galleries and in the open air; the latter method, although more expensive, is more profitable, in consequence of the great irregularity of the veins, which are thus more easily got at. All the emeralds now extracted are sent to Paris, where they are cut, an operation which causes them to lose more or less of their size and weight, according to the degree of crystallization and of purity. Of late years, too, this gem had obtained a fictitious value in France, on account of the color, green being that of the Empire. They were much in demand at the Imperial Court. It is impossible to determine the number of carats anually taken from the mines of Muzo. The production is very variable, depending upon the number of veins which may be found, and their richness. Whole months may pass without a single emerald being found, whilst 100,000 carats may be procured in a few days. It is also impossible to fix the mean value of the carat of emerald, as, when the stone is large, of a very dark color, and perfectly pure, which latter condition is extremely rare, it may be worked up to about £20 sterling a carat, whilst stones of a bright color, full of flaws, and divided into small fragments, are not worth 5s. a carat. These are called "Morallos," and have scarcely any value. There are no means of ascertaining the mean annual quantity of emeralds which has been previously or is now procured. This is the company's secret, which it preserves, from fear of the competition which will arise when it applies to Governmemt for a renewal of its lease. It is, however, estimated that, notwithstanding the expense of working, its financial condition is prosperous, and every one believes it will endeavor, in 1874, to secure that renewal. But there is no chance of the monopoly being preserved, since the present system is prejudicial to the treasury, as there are many mines which might be worked with profit, but which cannot be touched as matters stand. Without doubt, any great increase in the number of emeralds brought into the market would tend to lower that price; but it seems highly probable that English capital might hereafter be successfully employed in this branch of industry.

ON SOME NEW BUILDING MATERIALS.

From "The Engineer."

To make planks out of sawdust has hitherto been regarded a problem so insoluble that the expression has been used to point a jest, and referred to the same category as that to which belongs the art of spinning ropes out of sand. However, like so many other so-called impossible things, the manufacture of planks out of sawdust is now unquestionably possible, though we do not say economical; still the operation by which this might be accomplished, slightly varied, yields products not only curious but economical, and some of them, we believe, are destined to find large application as building materials.

Let us explain. The chemical material lignine, or cellulose, was regarded until quite recently as insoluble. Practically, everybody knows that timber of whatever species may be exposed to water for any length of time without suffering the slightest amount of loss by solution. Timber thus exposed may or may not decay—that will depend on the sort of timber, and also on the agents held in solution by the water of immersion; but it never dissolves. A more striking illustration, however, of the non-solubility of lignine or cellulose is seen in the long durability of cotton and linen goods. These may go to the wash again and again; when at the wash they have to suffer from one or more of the various nostrums which washerwomen delight in—alkali, plain or caustic, chloride of lime, soaps in variety. They have to withstand rubbing, boiling, ironing, mangling, and other hard usage; but the fabrics thus tortured never dissolve. They abrade and wear out mechanically, but that is all. The point need not be insisted on here, that every remark made concerning the non-solubility of woody matter, whether in the state of timber or of cotton and linen woven fabrics, equally applies to paper, which latter, although easily destructible by water soakage, is in water quite insoluble. Water can reduce paper to pulp, but that is all.

Having premised these remarks, we now come to the point of stating that the fluid "*cupro ammonium*" dissolves woody fibre with great facility. The rapidity of solution varies with the mechanical structure of the woody matter (cellulose or lignine) operated on. Old linen or old cotton textures that have suffered much wear and often gone to the wash dissolves in cupro ammonium, so to speak, immediately ; or, if a simile be required, they dissolve with very near the rapidity of a lump of sugar in a tumbler of hot water. New linen or new cotton are slightly more refractory, but still they dissolve with time. Similarly, and by a futher application of the same principle, sawdust, no matter of what wood, yields with more or less ease to this curious solvent.

There is good reason for belief that materials built up by taking advantage of this curious solvent property of cupro ammonium will before long be turned to great practical use, not, however, by effecting complete solution, but only partial or surface solution. Thus, to take a simple case, supposing it were desired to render a sheet of paper waterproof by cupro ammonium treatment, this could easily be effected in the following manner:—The sheet being dipped momentarily in cupro ammonium, and then passed between rolls to squeeze out excess of moisture, and finally dried, the paper so treated would be in a chemical, though not in a mechanical sense, waterproof. To be precise, such paper might be soaked indefinitely in water, even boiling water, without suffering any disintegration. Neither, if made into a bag and filled with water, would paper thus prepared allow any water to come through, save and except through such apertures as paper, even the best of paper, invariably possesses. Hence, to treat a single thickness of paper with cupro ammonium for the sake of waterproofing it, is an operation of very little use ; but if two thicknesses of paper be momentarily dipped in a cupro ammonium bath, then withdrawn and passed face to face between steel rollers, then the two surfaces adhere so absolutely that, when the compound material has dried, the plane of juncture is not only invisible, but cannot be rendered visible by any sort of dissection. Except under the condition that two holes in the two opposed sheets absolutely correspond, no defect of continuity can arise, and the chances against such correspondence of holes are almost infinite.

The manufacture of this double tissue paper furnishes the simplest case of what may be called lignine construction. What can be effected on two sheets can obviously be effected on any number by reduplication of the process, and thus artificial lignine sheets may be built up of any thickness, from that of paper to that of plank or scantling, should the operator so desire. The material, when in a certain state of moisture, moulds with almost the same facility as potters' clay. It readily corrugates, either by fluted rolling or by rectangular pressure, and the corrugated material, extremely light, hard and, chemically, next to indestructible, is destined, we believe, to supplant corrugated iron in numerous applications of the latter. So far as experience has gone, water exercises positively no influence on compound fabrics built up of lignine consolidated by cupro ammonium. Acids affect them only slightly, and not injuriously; in fact, the only agent which they cannot stand is ammonia.

With regard to cohesion, it is a remarkable, though not an unprecedented fact, that although cupro ammonium speedily effects solution of lignine, yet the first result of immersion is a strengthing of the fibre. If, for example, a piece of linen be tested for cohesive strength, and the result noted; if, then, a corresponding piece be dipped for an instant in cupro ammonium solution, next withdrawn, dried, and tested for cohesive force, this will always be found greater than before treatment. This result appears to be due to a contraction of the tissue by the chemical action, and suggests comparison with the curious accession of strength imparted to paper by instantaneous dipping in concentrated sulphuric acid, although a more prolonged immersion dissolves the same paper absolutely. The fact is very generally known that woody fibre—chemically speaking, lignine or cellulose—differs remarkably in cohesive tenacity, though all specimens of cellulose or lignine are identical as to chemical composition. If complete solution of any of these specimens be effected, the dried result is simply brittle semi-transparent glaze, the cohesion of which is no greater for the strongest original sample dissolved

than for the weakest. In point of fact, it is in no case practically desirable to effect complete solution, but only an incipient surface solution, whereby the original fibres, retaining their form and mutual arrangement, may become cemented together. Bearing this circumstance in mind, fabrics of extraordinary tensile and cohesive strength may be prepared by alternating canvas with paper, or, if preferred, attaching breadths of canvas face to face immediately. Thickness for thickness, we do not think that any timber can equal the strength of these compound fabrics.

For building uses there can be little doubt of the numerous applications these curious fabrics are destined to command. Thus, for roofage the very thinnest double tissue paper would not merely be water but wind-tight; and but for providing against the casualty of snow and other extraneous weights, nothing would be gained by using a thicker material. We have however, seen a specimen made of six thicknesses of common brown paper, and corrugated, which seems to us to be stout, strong, and reliable enough to be proof against all ordinary casualties. As regards ornamentation, it is worthy of note that the natural tint of some of these cupro ammonium lignine structures is very elegant. They readily take any sort of paint; but if painting be had recourse to they should previously be sized, otherwise the oil absorption effects an undesirable softening.

Although these remarks have been specially directed to the building applications of cupro ammonium fabrics, yet consideration of their properties will suggest many other utilities. Amongst these the manufacture of tubing is very important. A cheap, indestructible, and absolutely waterproof paper tube admits of more applications than can be summarily indicated. Amongst these the manufacture of cartridges for breech-loading arms and mining purposes stands prominent. Possibly thin sheets of this material may be turned to good account in hat-making —probably in boot-making. We have even seen a waterproof paper cape, the only obvious defect of which is a trifle too much of rigidity.

THE CHANNEL FERRY.

From "Engineering."

Simultaneously with the independent action of the French Government in the matter of International Communication, Mr. John Fowler's scheme for a Channel Ferry is again brought before the public, and is likely this year to receive more serious consideration than it has hitherto done. It is now seven years since a practical scheme to establish a service of steamers on a grand scale for the conveyance of passengers and goods between Dover and Calais was first made, and since that time the main features of the project have remained unchanged, although details have necessarily been altered.

After going through the usual preliminary delays, the Bill for the International Communication was passed in 1870, in the face of much opposition, and had it not been for the unfortunate complications on the Continent, which suddenly put an end to protracted negotiations, patiently followed through a host of difficulties up to a successful point, the works at Dover, as well as those on the French coast, would before now have been commenced. During the continuance of the war, however, and for many months after its conclusion, nothing could be done in the matter, at least with the co-operation of France, and it was therefore allowed to rest until a more favorable time.

In many respects it is well that this delay took place, because the project sanctioned in 1870 comprised a different arrangement of harbor at Dover to that orginally contemplated, and now proposed. The promoters of the scheme contemplated the construction of an independent harbor, owing to the opposition of the Dover municipality, but they have so far modified their scheme that at present they propose only to extend the Admiralty pier, and to construct a new pier, 4,000 ft. in length, together with a breakwater. By carrying out this plan the cost of works will be reduced, and the time required for construction will be

diminished. Moreover, the Dover Harbor Board, which was the principal opponent to the passing of the bill in 1870, will greatly benefit by the alteration.

At a recent meeting of the Dover Town Council Mr. Abernethy, in the absence of Mr. Fowler, stated that there were two main considerations. "First, the engineers had resolved to have no half measures, but to carry out a plan which would not require, as any half measures probably would, to be enlarged and altered hereafter, at loss of time, additional expense, and at the cost of hampering and hindering the development of the great traffic anticipated; secondly, it was desirable, in their opinion, for the commercial success of this undertaking, that provision should be made for carrying the trains across the Channel upon the steamers, so as to afford to passengers an unbroken through-train communication to Paris, and also that the same accommodation might be provided for goods."

With regard to the first of these considerations, there can exist no difference of opinion; the comparatively insignificant saving in first outlay would form no equivalent for the imperfection in the service that would necessarily follow; and the subsequent cost for the amplification or completion of the works would probably be greater in the end. Upon the second point, however, considerable misconception and difference of opinion exist, and it is upon this point that the Channel Ferry scheme is generally attacked.

While it is universally admitted that the present service falls short of the requirements of the existing traffic, and is, in fact, miserably deficient in comfort and facilities for passengers, it is urged that the proposition to effect an unbroken train communication between this country and the Continent is a useless refinement; and that the effects of motion, which it is not pretended could be entirely obviated by the adoption of the large boats, would be far more unpleasant to passengers sitting in railway carriages than to the same passengers in state-rooms. This statement is very true, but what of it? It is not proposed that the passengers should be shut up in their carriages; on the contrary, ample accommodation will be provided for them on board. The transfer of the carriages upon the vessel is an incidental advantage to the scheme, not a leading feature. The large boats Mr. Fowler has designed—450 ft. long and 57 ft. beam—are necessary for overcoming the heavy motion which the Channel seas impart to the smaller vessels, and the use of these large boats is the characteristic feature of Mr. Fowler's scheme—a feature that stamps its originality. No less are they necessary for receiving the goods wagons, which will be transferred from the railways to the rails laid in the lower part of the vessel, just as the passenger carriages are run upon its deck. Thus, to accommodate the goods traffic, and to obtain steadiness at sea for the benefit of the passengers, such vessels as the ones proposed (and to the efficiency of which Mr. Reed, Mr. Laird, Mr. Penn, and other well-known naval engineers have testified) are equally required.

Between the rows of saloons and state-rooms provided for the accommodation of passengers, there will be a wide space clear from end to end of the ferry, and it is rightly considered that this space could not be more appropriately utilized than by the passenger carriages, which will take comparatively little room, and add scarcely anything to the load of the boat. Thus the transfer of the train forms a natural part of the entire scheme, and nothing has been strained to achieve it. Had it presented any difficulties, could it only have been attained by a sacrifice of other requirements, or by a great increase in first cost, this part of the undertaking would never have been seriously considered. All travellers will fully appreciate the advantages arising from an unbroken railway communication with the Continent, and most would be willing to pay for the privilege of maintaining their carriage throughout were any charge demanded.

The inconvenience of shifting from the train to the boat at Dover, and going through the reverse operation on the French side, burdened with all the minor *impedimenta* of a journey, are too well known and appreciated to be enlarged upon, and we think that the travelling public can have but one opinion on this point. Mr. Fowler will save all this inconvenience by transferring the train, and although neither he nor any one can altogether arrest the evils of sea-sickness,

his large boats will reduce it, and the more sensitive who succumb even under the modified condition of things, will at last be spared the infliction of climbing the wet and slippery steps of the Calais jetties, and can retreat to their carriage so soon as the smooth water of the harbor gives them confidence to quit their state-rooms.

It is urged that insurmountable difficulties will be encountered during bad weather in running the trains upon the ferry, and in securing them afterwards. We think that a sufficient answer to these objections is supplied by the fact that Mr. Fowler has pronounced all the details of the undertaking to be practicable, and, indeed, easy. It is certain that experience will before many years settle these and all other points connected with the Channel Ferry, for the works connected with it cannot be much longer delayed. Public opinion becomes more and more decided against a continuance of the present unsatisfactory mode of transit, and money will be plentifully subscribed towards the completion of such a national work.

THE GRADIENTER.

By BENJ. SMITH LYMAN.

Written for Van Nostrand's Magazine.

Having seen in the last number of your excellent magazine, a very favorable account of the Gradienter and its use, I beg to point out what seems to me to be some serious defects of the method as compared with common vertical circle levelling.

In the first place the "percentage screw," the main feature of the instrument, is liable to wear, so that after some use it can be turned to a notable amount without moving the telescope, and so leads to a wrong reading.

In the next place, the friction and weight of the telescope and its axis give so much resistance to the percentage screw, as to strain it more or less, and tend to make its readings slightly inexact.

The error from these two sources might very well amount to so small a quantity as half a minute of arc or even a whole minute; and it is not therefore suprising that, even "in testing the instrument" while it must have been comparatively new, "at a distance of 500 ft. the elevations as determined by the percentage screw would," sometimes though "seldom vary more than an inch from true levels." In a transit with a vertical circle or arc of a radius of two inches and a half or three inches, the vernier marks single minutes and it is very easy to read to half a minute. The sine of half a minute of vertical arc at a distance of 500 ft., is at most but nine-tenths of an inch. As the truth cannot be more than half way from the nearest possible reading to the next, not more, that is, than a quarter of a minute from the actual reading, the greatest possible error from this source is less than half an inch of level in a distance 500ft. It is clear, then, that the common vertical circle is much more exact than the percentage screw.

The vertical circle is also used far more conveniently and rapidly. It is claimed that the gradienter enables the reading of heights of 40 or 50 ft. above the level, which at a distance of 500 ft. would be a vertical angle of only thirty-five minutes; while we have just seen that the vertical circle can be used without lessening its exactness through any arc. Indeed the increase of the length of the sine of angles is so regular even up to forty-five deg., that graduations could be made if desired, so as to read direct with a vernier the height for 100 ft. of distance, and the number of feet above or below level could then be got by multiplication, as in using the gradienter. But it is perhaps more convenient to add the logarithm of the sine of the vertical angle to the logarithm of the number of feet of distance and so find the difference of level.

Moreover, the vertical circle requires no special adjustment at each station, answering to the adjustment of the zero of the percentage screw to the zero of its scale; a saving of one source of care and error that is not unimportant.

With the vertical circle, too, the telescope can just as well be sighted at a target, of one height always (say about that of the telescope), and then there is no

need of the many additions and subtractions that are described for the most convenient use of the percentage screw, and that lead to confusion and mistakes.

If, then, the gradienter is convenient and quick, how much more so must the common vertical circle be. It is needless to speak here particularly of the skilful work and great care that must be laid out to make the percentage screw properly; and all to no purpose it seems.

Perhaps the commonest source of error in using thr vertical circle is gross misreading, especially reading the wrong way from a numbered graduation or from the zero; for example, reading twelve deg. instead of eight, or mistaking up one deg. when ten is marked for down one deg. Such mistakes come of course from having the numbering made in one direction for a rising angle and the other way for a falling one; and they could be done away with by having a continuous numbering for both angles, so that the reading should always be in one direction. For instance let the reading for a level sight be zero; for ten deg. rise, 350 deg.; and for ten deg. fall, 10 deg.; or take the numbers either side of 180 deg. The sines of the angles would be the same as in the common graduation; and no special additional mark in the note book would be needed to show whether the sight is a rising or falling one. There is plainly the same motive for having but one continuous graduation from zero to 360 deg. on the main plate and compass ring, and it is well arranged so on the gradienter. Having the numbers run different ways in different quadrants is a most fruitful source of error.

The use of the stadia as described with the gradienter, is doubtless a great saving of labor and time, and even a great gain in exactness over common chaining. But instead of having a rod graduated expressly for the stadia wires, it is much more convenient to have the wires adjustable by the surveyor, or adjusted by the maker so that they cut off a foot on the rod at a 100 ft. away, and the distance can be read direct from a level rod (marked in feet and hundredths), with the same sight as for the level or for the vertical angle. The eye piece of the new transit of Messrs. Heller & Brightly, of Philadelphia, is remarkably well suited from the flatness of its field, for stadia measurement.

THE EFFLUX OF ELASTIC FLUIDS.

From "Engineering."

Numerous experiments have been made from time to time since the days of Galileo for the purpose of determining the laws governing the flow of elastic fluids. During this lengthened period it has been very generally thought, and is still largely considered at the present time, that the same laws govern the flow both of elastic and inelastic fluids. We are aware that there have been experimenters, some of them able men, who have occasionally startled us by boldly announcing, as the results of experiments, theories totally different and opposed to that generally accepted. Some years ago an experimenter announced a theory to the effect that elastic fluids under a great or small heat of pressure could not flow into a vacuum. Another experimenter advocated a similar theory in France. More recently an experimenter declares and maintains that the velocity of efflux is the same whatever may be the recipient pressure, so long as the latter is less than half that constituting the head of pressure. So that atmospheric air at a pressure of 15 lbs. on the sq. in. would flow with the same velocity into a vacuum as into a recipient pressure equal to, or less than, 7½ lbs. on the sq. in. These, amongst other examples of a similar character, show the difficulties in finding a method of experimenting with elastic fluids under heads of pressure sufficiently great to be of a use in practice, and which is either free from those well-known changes which take place, due to the relations existing between the densities, temperatures, pressures, and volumes of elastic fluids in movement, or by which their effects can be accurately taken into account.

The greatest number of the experiments most relied upon have, however, been made on the efflux of elastic fluids under small heads of pressure, varying up to about 6 in. of water, and it is mainly

the results of such experiments which have induced many scientific men to believe that elastic and inelastic fluids are governed by the same laws in their flow.

The laws affecting the flows of elastic fluids are of very great importance both to engineers and physicists. Every steam or other heat engine from the fire-bars to the flue or chimney in one direction, and from the boiler to the condenser in the other, consists of little more than numerous passages for the flow of elastic fluids of various densities and temperatures, and hence the importance of knowing the true laws which govern the flow of these fluids. The ventilation of mines, blast, and regenerative furnaces, the elastic fluids evolved from exploded gunpowder in guns, and from other explosive compounds used for various purposes, are, also, important matters depending on the laws of the flow of elastic fluids for a proper solution of effects. Considered purely in a physical sense, this question is not of less importance. During the last few years many earnest laborers have greatly extended our knowledge—as they are still doing—in connection with the elastic fluids composing the atmosphere of the sun, and the effects of their movements in causing the various phenomena which have recently been observed. The atomic action of matter is at present totally unknown, and it is more than probable that the first gleamings leading to a solution of that mystery will be through the action of matter in an elastic state.

Under these circumstances we have great pleasure in informing our readers that some extensive experiments and researches on the efflux of elastic fluids have recently been made by Mr. George Wilson, C. E., of Parliament street, Westminster; that the results of these experiments have been arranged for publication in a form suitable for the use of practical men, and that the results thus arranged will appear in this journal.

These experiments, which have been carried out on both air and steam, have been made by Mr. Wilson by three different methods and apparatus. The heads of the pressures of efflux commence as low as two-tenths of an inch of water, and are increased by fractions up to 1 in. of water, and then by consecutive inches up to 14 in. of water. The heads of pressure of efflux then commence at 1 in. of mercury, and are increased by consecutive inches up to 24 in. of mercury, and finally by degrees up to 100 lbs. on the sq. in. There is, thus, a gradual increase in the heads of pressure of efflux from that equal to two-tenths of an inch of water up to 100 lbs. on the sq. in. The efflux orifices consist of short tubes, one, two, and three centrimetres in diameter, altered for different series of experiments until they finally had the form of the contracted vein. A series of experiments have also been made with the same apparatus and efflux tubes with water for the purpose of comparison. It will be seen that these experiments have been made on that extensive scale which has so long been needed to show effectually the laws of efflux under different heads of pressure.

The important results which have been derived from these experiments, induced Mr. Wilson to extend his researches and examine all those made by various experimenters on the efflux of elastic fluids during the last 200 years, for the purpose of ascertaining the accuracy or defects of their methods of operating. Many of such experiments are published in the "Transactions" of the literary societies in England and on the Continent, and are consequently not easy of access to many persons, nor are they generally known. We hope to be enabled to place before our readers these valuable researches of Mr. Wilson at some future time.

As the period of the Transit of Venus in 1874 approaches, astronomers both at home and abroad are becoming more and more active in their preparations; and the American committee on this subject, it is understood, has already decided in considerable part upon the stations to be occupied. In Russia, the committee, under Prof. Struve, proposes the establishment of a chain of observers, at positions 100 miles apart, along the region comprised between Kamschatka and the Black Sea. The German committee has decided on recommending the organization of four stations for heliometric observations of the planet during its transit, one of them in Japan or China, and the others probably at Mauritius, Kerguelen, and Auckland Islands.

NARROW GAUGE vs. WIDE GAUGE.

By C. J. QUETIL, C. E., Principal Assist. Eng. Texas Pacific R. R.

Written for Van Nostrand's Magazine.

I have been directed by Gen. Geo. P. Buell, Chief Engineer of the Texas Pacific Railroad, to prepare an answer to Mr. Silas Seymour's attack on his (Gen. Buell's) late report to the Directors of the road, his official duties allowing no time for the performance of such a work.

I feel much honored by the confidence thus reposed in me by the Chief Engineer, and have tried to prove worthy of it.

The following article was prepared some time since and was submitted to Gen. Buell for approval. With his permission it is offered for publication.

The article is not prepared with the intention of influencing in any way the Directors of the Texas Pacific Railroad. The Legislature of Texas have decided that for that State the gauge shall not be less than 4 ft. 8½ in., and so far as my knowledge extends, the decision was made before Mr. Marshall O. Roberts requested of Mr. Seymour his opinion on the subject of the gauges.

The object of the following is merely to answer the attacks of Mr. Seymour on Gen. Buell's report.

The work would doubtless have been better performed by Gen. Buell himself, but new duties, as General in the army, demanded his attention, and the honor and duty of the reply was assigned to me.

It is not necessary, in comparing the cost of a wide-gauge road-bed with that of a narrow gauge, to observe the conditions indicated by Mr. Silas Seymour, in his pamphlet (p. 7).

Let us suppose that the average height of embankments of the two roads is 6 ft. and let us compare the sections of embankments of the two roads, one built for the 4 ft. 8½ in. gauge, the other for the 3 ft. or 3 ft. 6 in. gauge.

This will allow us to make a fair estimate of the cost of earthwork.

Let us first state that in making his report, Gen. Buell has compared the section of a road-bed embankment 14 ft. wide at top, as they are generally built for the 4 ft. 8½ in. gauge, with that of an embankment 9 ft. wide at top, corresponding to a narrow-gauge road.

Gen. Buell never pretended to establish or prove that a road built with 10 ft. 9½ in. width of embankment, or even with 10 ft. width of embankment, would cost 30 per cent. less than a wide-gauge road-bed, and Mr. S., in his review, does not compare the same sections of embankment as Gen. Buell has compared, and, naturally enough, he comes to a different result.

I am going to prove that Gen. Buell's assertions are right, not only for the economy of earthwork in favor of the narrow gauge, but also for the clearing and grubbing, hauling, drain boxes, culverts, bridges, superstructure, economy of dead weight in cars, and in fact all the points indicated in Gen. Buell's report.

I shall not assume, like Mr. S., that the width of bank of wide-gauge roads (4 ft 8½ in.) which is generally 14 ft., might be 12 ft., but I will take it, as it is, at 14 ft., and in my answer to Mr. S. I will not compare what might exist in wide-gauge roads with what exists or might exist on narrow-gauge roads, but I will compare what actually exists on one system of road with what exists on the other, and in doing so, if I do not convert people to the narrow-gauge theory, but simply point to possible reforms in road-bed, and in cars, that might be made on wide-gauge roads, my work will not have been made in vain.

Let us compare the section of road-bed 6 ft. high with 1½ to 1 slope, and one of them 14 ft. wide at top, while the other is only 9 ft. wide at top.

The banks will have bottom widths of 32 ft. and 27 ft., respectively.

The difference between the two sections is 30 sq. ft., or very near 30 per cent. of the section of the narrow gauge, and as the cost of earthwork is proportional to the sections of the embankments, the slopes being the same, is it not fair to say that the difference of earthwork will be 30 per cent. of the cost of narrow gauge?

CLEARING AND GRUBBING.

I claim also that the difference between the cost of clearing and grubbing for the two roads is in the proportion of 62 to 57, as it is customary to clear 15 ft. each side

outside of the slope stake. The difference between 62 and 57 is 5, which is more than 8½ per cent. of the cost of narrow gauge.

DRAIN BOXES.

The length of the drain boxes will be reduced in the proportion of 32 to 27, or very near 19 per cent. of the cost of narrow-gauge drain boxes.

BRIDGES.

As for bridges, it may be that some of those built on wide-gauge roads and braced on a wrong plan, gave way on account of deficient lateral bracing, but the generality of those destroyed have been burnt or broken down by rupture of the chords, the lateral bracing having nothing to do with it; and I claim that the lateral bracing in the generality of the wide-gauge bridges, 16 ft. wide, answer the purpose, and that the bridges for narrow-gauge roads can be made 12 ft. wide instead of 16 ft., with a lateral bracing sufficiently strong, the light stock running on a narrow-gauge road having considerably less effect to put the bridge out of line than the heavy stock running on a wide-gauge road. Consequently the economy for bridges and culverts will be found to be 33 per cent. of the cost of bridges on narrow-gauge roads. In using cars 8 ft. or even 8 ft. 6 in. wide, there would be room enough each side to allow a man to stand without being hurt, and that is all that is needed.

LOOSE AND SOLID ROCK AND HAULING.

There will be also an economy in loose rocks, solid rocks, and hauling. In making the cuts 12 ft. wide instead of 18 ft., as they are on wide gauge, it can be seen that the difference between two cuts, 5.4 ft. high, and of very near the same section as the embankments above spoken of, will be 32.4 sq. ft., or about 30 per cent. of cost of narrow gauge.

Recapitulating, I have proved that there is in narrow-gauge roads an economy of:

Earthwork, 30 per cent. of cost of narrow gauge, or 22 per cent. of cost of wide gauge.

Clearing and grubbing, 8½ per cent. of cost of narrow gauge, and 8 per cent. of cost of wide gauge.

Drain boxes, 19 per cent. of cost of narrow gauge, and 16 per cent. of cost of wide gauge.

Bridges and culverts, 33 per cent. of cost of narrow gauge, and 25 per cent. of cost of wide gauge.

Loose and solid rock and hauling, 30 per cent. of cost of narrow gauge and 23 per cent. of cost of wide gauge.

Let us see now what economy per mile the Texas Pacific could insure by building the road for a narrow gauge, with embankments 9 ft. wide at top, and excavations 12 ft. wide at bottom.

The cost per mile between Camp's Ferry and Tyler, on the Southern Pacific, is

Clearing and grubbing	$366 00
Earthwork	7,766 15
Loose rocks	1,070 25
Hauling	2,336 20
Culverts and bridges	4,968 17

The economy would be

Clearing and grubbing	$29 28
Earthwork	1,708 55
Loose rocks	246 15
Hauling	537 33
Culverts and bridges	1,242 04
Total per mile	$3,763 35

And as it is calculated that the Texas Pacific will be at least 2,000 miles long, the economy for the whole length of the road would be $7,526,700 (seven millions five hundred and twenty six thousand and seven hundred dollars).

Now if the question is asked why we leave less side space on a narrow-gauge road than a wide-gauge one, we answer that it is because we think that the wide-gauge roads have got too much of it; but even in making the embankments 10 ft. wide, the cuts 14 ft. wide and the bridges and culverts 14 ft. wide, to which dimensions Mr. S. does not object, I am going to prove that the economy would be yet very great in comparison with embankments 14 ft. wide at top, cuts 18 ft. wide, and bridges 16 ft. wide. In supposing the embankments 6 ft. high, as before, the section of the wide-gauge road-bed will be, as before, 138 sq. ft., and that of the narrow gauge, 114 sq. ft. The difference between the two being 24 sq. ft., or 16 per cent. of the section or cost of wide-gauge embankment.

The difference between the clearing and grubbing on the two roads will be 62 − 58 = 4, or 6½ per cent. of the cost of grubbing on wide gauge. The difference for bridges and culverts will be 12½ per cent. of their cost on the wide gauge. The economy for loose rocks, solid rock, and

hauling, will be 16 per cent. of the cost on wide gauge.

Consequently, the economy would be per mile

Clearing and grubbing	$23 79
Earthwork	1,242 58
Loose rocks	171 24
Hauling	373 79
Culverts and bridges	621 02
Total per mile	$2,432 42

or $4,864,840 for the 2,000 miles of the road.

This is the saving that could be effected on the road-bed of the Texas Pacific, by building the banks 10 ft. wide, the excavations 14 ft. wide, and also the bridges 14 ft. wide; and I appeal to all engineers who will, like me, examine the question honestly, to decide if it is not so.

The saving of $4,864,840 is a very great one, and so far from weakening our side of the question, it is, on the contrary, a very strong argument in favor of the narrow gauge.

Mr. S., in his review, has not spoken of the economy in clearing and grubbing loose and solid rocks and hauling. We have not made the same omission.

We therefore consider that we have carried our first point, the economy in construction of road-bed, which is, if the banks are 9 ft. wide and the cuts 12 ft., equal to 23 per cent. of the cost of wide-gauge roads; and, in the second case, if the banks are 10 ft. wide, cuts 14 ft. wide, and bridges 14 ft. wide, is equal to 15 per cent of the cost of wide-gauge roads. According to Mr. S., this would be sufficient to decide in favor of the narrow gauge; but we will go farther. We expect to prove that on the other points we are also right.

By a process of arguing similar to the one he used for denying the importance of economy in the road-bed, Mr. S. tries to show that the economy claimed by Gen. Buell for cost of superstructure is equally imaginary. He says, on page 9 of his pamphlet: "The requisite weight of iron rails is generally supposed to be governed by the weight resting on each driver of the engine, and as this weight creates the adhesion, and therefore governs the power, it follows that with the same weight of train, it must be equal on both gauges." This would be true if, on narrow-gauge roads, to haul a certain amount of live load they were using cars having a proportion of dead weight to the live weight or capacity as great as that of the cars used on wide-gauge roads; but this is far from being the case, as on narrow gauge they can haul the same amount of live load as on a wide-gauge road with a lighter train, with less dead weight of cars, although using a greater number of them, and consequently are able to run them on a lighter rail, as we will prove hereafter.

The rail used on the Denver and Rio Grande, and most of the 3-ft. gauge roads, is a 30-lbs. rail. Gen. Buell recommended a 36 lbs. rail for a 3 ft. 6 in. gauge. We will show that the economy of superstructure amounts to something more than the reduced length of the ties. If the rails are lighter than the fish plates, spikes and bolts will also be lighter, and the ballast will be used in smaller quantity.

We give here the comparative estimate of the superstructure of two roads, one with a 4 ft. 8½ in. gauge, the other with a 3 ft. 6 in. gauge. In the former we will suppose the rail to weigh 55 lbs., and in the latter 35 lbs. per yard.

Cost of superstructure for 4′ 8½″ gauge, 55 lbs. rail.

87 tons of rails at $70 per ton	$6,090
400 rail splices at $1	400
5,500 lbs. of spikes at 5c	275
2,640 cross ties at 80 cents	2,112
2,000 C yds. of gravel ballast at 50 cts.	1,000
Laying one mile of track	500
Total per mile	$10,377

Cost of superstructure of 3′ 6″ gauge, 35 lbs. rail.

55 tons of rails at $75 a ton	$4,125 00
528 tons of rail splices at 60 cents	316 80
3,520 lbs. spikes at 6 cts.	211 20
3,520 cross ties 5′×5″×6′ at 40 cents	1,408 00
1,200 C. yds. gravel ballast at 50 cents	600 00
Laying one mile of track	350 00
Total per mile	$7,011 00

Mr. Seymour, who thinks that the economy for the superstructure is only in the ties, will observe that we put more ties on the narrow gauge than on the wide one, and that the economy of superstructure is $3,366 per mile, or very near 45 per cent. of the cost of superstructure of narrow gauge, and 32 per cent. of the cost of superstructure of wide gauge. From this estimate it will be seen that the Texas Pacific Company, in building a 3 ft. 6 in. gauge road, might economize 2,000 times $3,366, or $6,732,000 in the superstruc-

ture. This, added to the economy on the road-bed in building the banks 9 ft., would make a total economy of $14,258,700, and in case the banks should be built 10 ft. wide, an economy of $11,596,840, and this, on ordinary railroad ground. It is principally in countries like New Mexico, Arizona, and California, which the line will have to cross, that the Company might make an immense saving by adopting the narrow gauge.

The adoption of a narrow gauge allows sharper curves for the line, and saves, furthermore, much of the heavy expense of tunnelling.

Herewith we give some important reports bearing directly upon the points at issue.

Gen. Wm. J. Palmer,
Pres't D. & R. G. R. R. Co.

Dear Sir—I have the honor to submit the following comparative estimate of cost for a broad and narrow gauge road from Golden City to Blackhawk, distance 20 miles :

BROAD GAUGE 4 ft. 8 ½ in		NARROW GAUGE 3 ft.		Difference in favor of narrow gauge.
	Per mile.		Per mile.	Per mile.
Golden City to mouth of North Fork of Clear Creek, 13 miles, max. grade, 125 ft., max. curve, 12°; from North Fork to Blackhawk 7 miles, max. grade 155 ft., max. curve 12°.		Golden City to Blackhawk, distance 20 miles, maximum grade 175 ft., maximum curvation 26°.		
Graduation, masonry, bridging and ballasting	$46,425 00	Graduation, masonry, bridging and ballasting	$7,200 00	$39,225 00
Superstructure, track, etc	13,610 00	Superstructure, track, etc	8,765 00	4,845 00
Depot buildings, repair shop, tanks,	1,220 00	Depot buildings, repair shop, tanks	1,000 00	220 00
Engineering superintendence and contingencies	4,500 00	Engineering, superintendence and contingencies	1,500 00	3,000 00
Total cost per mile	$65,755 00	Total cost per mile	$18,465 00	$47,290 00

Cost of broad gauge per mile	$65,755
" narrow " "	18,465
Difference in cost per mile	$47,290
Cost of broad gauge for 20 miles	$1,315,100
" narrow " " 20 miles	369,300
Difference in cost for 20 miles	$945,800

The Denver and Rio Grande Railway use

For width of road-bed in cuts	12 ft.
" " " in banks	10 ft.
Sharpest curve	6°
Maximum grade (for nearly 30 miles)	75 ft. per mile.
Length of ties	6½ ft.
Weight of iron	30 lbs. per yd.
Slope of earth cuttings	1 to 1.
" banks	1½ to 1.
" solid rock cuttings	¼ to 1.

These estimates are made from careful surveys, the most of the lines being actual locations; the curved lines having been run at all the most difficult points.

Having no reliable data from which to estimate the cost of rolling stock, etc., particularly the motive power for operating on grades of 150 to 175 ft. per mile with the maximum curvature of 26 deg. I have not given an estimate for equipment.

Estimated weight for iron rails as follows :

For broad gauge	65 lbs. per yard.
" narrow "	40 " "

There would appear to be a discrepancy in the engineering and contingency estimate (by comparison), but is accounted for by the difference in length of time to construct; there being unavoidably 3 tunnels on the broad gauge (if only 12-deg. curves are used), which would keep up an organization for engineering and superintendence 2 years instead of 6 or 8 months, in which the narrow gauge can be completed.

The extension of line from Blackhawk to Central City can be made at the average cost for narrow gauge given, distance about 7 miles; but I think it hardly possible to build for broad gauge with 12 deg. curves. Very respectfully,

(Signed) J. P. Mersereau,
Chief Eng'r.

UNION PACIFIC R. R. Co.
GENERAL SUPERINTENDENT'S OFFICE,
OMAHA, NEBRASKA, *Nov.* 9, 1871.

MY DEAR SIR,—I inclose you the Directory with blanks filled up as requested.

An approximate location has been made during the last two months from Pine Bluff to Nevada, Cal., and the line has been definitely located from Golden City to the forks of Clear Creek, 13½ miles. Of this distance 5 miles have been graded since the 1st of September, on which occurs the heaviest work through the cañon. On this 13½ miles the creek falls 1,700 ft. The cost of grading a road-bed through the cañon for a 4 ft. 8½ in. track was estimated to be $90,000 *per mile.* The *actual cost* of grading a road-bed for a 3 *ft. track* has not exceeded $20,000 *per mile.*

It is expected that the road will be completed from Pine Bluffs to Nevada by the 1st of September next, and a branch 22 miles long from the forks of the creek to Georgetown, by Jan. 1st, 1873. A third rail will be laid on the ties between Denver and Golden City, 15 miles, making the entire length of Colorado Central R. R. and branches—192 miles narrow gauge and 15 miles of 4 ft. 8½ in. gauge.

Respectfully yours,
(Signed) T. E. SICKELS,*
Ch'f Eng'r C. C. R. R.

To Dr. R. H. LAMBORN,
Vice Pres't D. & R. G. R. R.

Considering the saving announced by Mr. T. E. Sickels as somewhat extraordinary, I wrote to that gentleman to ask him some explanations, and obtained the following reply:

UNION PACIFIC R. R. Co.
GENERAL SUPERINTENDENT'S OFFICE,
OMAHA, NEBRASKA, *Nov.* 28, 1871.

CH. J. QUETIL, Esq.,
Ass't Eng'r Texas Pacific R.R., N. Y.

DEAR SIR,—I have received your letter of 20th instant. The large difference in the cost of grading for a broad-gauge and for a narrow-gauge railroad through Clear Creek Cañon, Colorado, results from the fact that the location of the two lines occupy different ground.

On the broad-gauge location the minimum radius of curvature adopted was 955 ft., and on the narrow gauge it is 220 ft. The cañon is about 3,000 ft. deep and has precipitous sides. Its course is so tortuous that the broad-gauge location would have required in construction numerous tunnels and bridges across the stream, with high embankments and deep open rock cuttings. The adoption of a narrow gauge admitted of an alignment conforming approximately to the windings of the cañon, and enabled us to obtain a graded road-bed for less than ¼ of the estimated cost of a broad-gauge road-bed, with the additional advantage that increase of distance secured more favorable grades.

Yours truly,
T. E. SICKELS,
Ch'f Eng'r C. C. R. R.

I leave it to the stockholders of the Texas Pacific to decide what an immense saving on the cost of the road might be made in the tortuous hills of Arizona and California, by adopting a narrow gauge.

Mr. S. in his review shows his complete ignorance of the dimensions of cars used on narrow-gauge. He assumes that the cars of a narrow-gauge road will be only 1 ft. 2½ in. smaller in width than those of the wide-gauge and allows for no reduction in length.

He says: "The saving in the cost of each car will be only the value of a longitudinal section of 1 ft. 2½ in. taken from the centre of the car, embracing only the top, bottom, and two ends of the body, and the truck frames and axles below, and perhaps a still further trifling deduction on account of the value of materials, and labor claimed to be saved in the construction of cars of the proposed diminutive width; but I claim that the additional number of cars required to transport the same amount of tonnage, or number of passengers, will make the cost quite as much if not more, for the narrow than for the wider gauge."

So that Mr. S., in writing that new sentence, thinks or tries to make people think that to make a narrow-gauge car, it is only necessary to make it of the same length and 1 ft. 2½ in. narrower than those used on wide-gauge roads. By assuming this, he not only proves that he has not read the report of Gen. Buell, who gives the dimensions of narrow-gauge cars, but also that he lacks the first knowledge necessary to discuss the subject, viz.:

* Gen'l Sup't Union Pacific R. R.

the knowledge of dimensions of cars used on narrow gauge. He thinks (p. 13) that we ought to have gone to the New York Central and Erie Railway depots to learn something about dimensions and capacities of cars. We will give him the advice to visit at Wilmington, Delaware, the large car shops of Jackson & Sharp, and those of Harlan & Hollingworth, and also those of Billmeyer & Small, at York, Pa. Those establishments are now making new cars for about 20 narrow-gauge railroads, and Mr. S. will be able to learn something about their dimensions.

If he wants to see the cars running and how they stand the gales of Colorado, and also what amount of live load they are able to carry, he can complete his travel by visiting the Denver and Rio Grande Railroad.

He will learn that Jackson & Sharp are building for narrow-gauge passenger cars 35 ft. long, 7 ft. wide, and 16 ft. 6 in. high, to carry 36 passengers, instead of cars 48 ft. long in body, and 54 ft. long over platform, 9 ft. 6 in. wide, 7 ft. 10 in. high at sides, and 10 ft. 3 in. high at dome, and built for 60 passengers, and on the 4 ft. 8½ in. gauge roads.

Also that Billmeyer & Small are building platforms (4-wheeled) 12 ft. 6 in. long and 6 ft. wide, and some others 16 ft. 6 in. long.

Also 8-wheeled platform cars, 23 ft. 6 in. long and 6 ft. wide, instead of 32 ft. long and 9 ft. wide, as on the wide-gauge roads.

Also 4-wheeled box cars 10, 12, or 14 ft. long, and 6 ft. wide.

Also 8-wheeled box cars, 22 ft. long and 6 ft. 6 in. wide, instead of 29 ft. long and 9 ft. wide, as they are built for 4 ft. 8½ in. gauge roads. He will be able to satisfy himself that 8-wheel box cars, same length as that shown in Gen. Buell's report, and only 1 ft. narrower, and with 2 trucks of 4 wheels each, and of same form as the trucks indicated in Gen. Buell's report, are built at Billmeyer & Small's, York, Pa., for the Denver and Rio Grande Railroad, and that I have in Gen. Buell's office the photograph of one of those cars bearing "No. 2,001, D. & R. G. R. W.," and that consequently his assertion (p. 13 of his pamphlet) that the box car indicated in Gen. Buell's report is "only in theory and upon paper," is erroneous, and shows still more his ignorance of the facts, as the same car which is built 6 ft. 6 in. wide could certainly be built 1 ft. wider and run under the same conditions.

We will complete our informations to Mr. S. by adding some statistics of the weight and capacity of these cars.

Harlan & Hollingworth are building for 3 ft. gauge roads box cars 14 ft. long, 6 ft. wide, 7 ft. 6 in. high, weighing empty 4,500 lbs., and with a capacity of 8,000 lbs. Those cars are running on the Denver & Rio Grande R.R. The 4 wheeled box cars with box 10 ft. long, frame 12½ ft. long and 6 ft. wide, built by Billmeyer & Small, weigh 4,500 lbs. empty and have a capacity of 9,500 lbs.

Their 4 wheeled flat cars, whose dimensions I gave before, weigh 3,500 lbs. empty and have a capacity of 10,500 lbs. They are also running on the Denver & Rio Grande R.R. The 8 wheeled platforms weigh 6,250 lbs. empty and have a capacity of 20,000 lbs. The 8 wheeled box cars weigh empty 8,800 lbs. and have a capacity of 16,000 lbs.

The passenger cars built by Jackson & Sharp for 36 passengers weigh 15,000 lbs., the dead weight per passenger being 416 lbs.

Let us now compare the proportion of dead weight to live weight in the narrow gauge cars with this of the 4 ft. 8½ in. gauge cars.

For Narrow Gauge:

4 wheeled flat cars..... 3,500 proportion 33.3 per cent.

8 wheeled do. ..... 10,500 / 6,250 " 31.2 "

4 wheeled box cars..... 20,000 / 4,500 " 47.3 "

8 " " " ..... 9,500 ..55 "

8 wheeled passenger cars for 36 passengers, dead weight per passenger 416 lbs.

For 4' 8½" Gauge.

8 wheeled flat cars 32 ft. × 9 ft. 16,500 / 22,400 proport'n 73.6 per cent.

8 wheeled box cars 29 ft. × 9 ft. 18,000 / 22,400 proport'n 80.4 per cent.

8 wheeled passenger cars for 60 passengers 48 ft. long by 9 ft. 6 in wide..... (39,000 / 7,200) Dead weight per passenger equal to 650 lbs.

Consequently, there is in favor of the narrow gauge an economy of dead weight of:

For flat cars..... ..... 42.4 per cent.
For box cars..... 33.1 and 25.4 per cent.
For passenge cars ..... 234 lbs. per passenger.

Page 14 of his review Mr. S. says: "And

will he or any other advocates of this extreme narrow gauge theory, undertake to demonstrate why a platform 10 ft. square, and capable of upholding a given maximum weight, should necessarily be of more than twice the weight and strength of one 10 ft. long and 5 ft. wide, and capable of sustaining just one half of the same maximum weight."

In asking such a question, Mr. S. shows, I repeat it again, his ignorance of the way the narrow-gauge cars are built.

He assumes that to build a narrow-gauge car platform it is only necessary to cut in two, lengthwise, the platform of a broad-gauge car, and to move only one of the inside sills to make of it the outside sill of the new platform, reducing the width only.

Of course, if such a platform was cut in two equal parts in the line of its longitudinal axis, each of the parts ought not to weigh more than the half of the weight of the first platform; but this misrepresents the true proportions of narrow-gauge cars, and conceals the fact that these cars can carry the same load with less weight of car than the broad gauges.

To enable Mr. S. to understand this, we will offer some remarks on strength of materials.

If a beam or sill of a given length, and supported at each end, is able to carry a certain weight, when I cut the beam or sill in two parts, equal in length, and support one of those part, at each end, this half beam or sill, with half the material and weight of the first one, will support double the weight that the first one could carry. In other terms, if a beam 12 ft. long, 2 in. broad, and 4 in. deep, can, when supported at each end, carry 1,000 lbs., another beam 6 ft. long, same width and same depth as the preceding, will be able when supported at each end to carry 2,000 lbs.

Now, what is a platform? It is a flooring supported by a certain number of longitudinal and transverse beams or sills. What support at each end, or very near it, those longitudinal beams or sills? The trucks of the car. Consequently, if the platform 10 sq. ft., spoken of by Mr. S., has 4 longitudinal sills, and is able to carry 4,000 lbs., each of those sills will be able to carry 1,000 lbs. If we cut them across in two equal parts, each of those new sills 5 ft. long will be able to carry 2,000 lbs. and we may have a new platform 5 ft. long and 10 ft. wide which will weigh half as much as the first one and will be able to carry double the weight. This is the whole secret of the economy of dead weight that the narrow-gauge car builders effect in reducing the length of the sills of the cars and not their width, without reducing their length, as Mr. S. would lead his readers to believe. On what narrow-gauge road or which narrow-gauge car shop has Mr. S. seen builders reducing the width of the cars used on wide-gauge roads without reducing at the same time their length? Does he think that car builders would any way reduce the width of the cars without reducing also their length? They know better. They know that it is principally in reducing the length of the cars that the economy of dead weight is effected. If in the above mentioned platform, 5 ft. long by 10 ft. wide, we diminish by one half the width of each of the four sills, we shall have a platform weighing a little more than one-fourth of the first platform and able to carry the same weight. This diminished weight of the platform will allow us to run it on smaller wheels, and consequently to lower its centre of gravity. The centre of gravity being lowered, it follows as a natural consequence, that the gauge might be reduced, and the platform run with the same stability, as one on a 4 ft. 8½ in. gauge running on larger wheels. So far, in building for narrow-gauge roads, the car builders have only succeeded in making a reduction of weight of the half or the two-thirds of the weight of the 4 ft. 8½ in. gauge cars for carrying the same live load; but this result is satisfactory enough. The broad gauge advocates will now say that they could build for their roads cars just as small as those used on narrow gauge and with the same proportion of dead weight to live weight. We admit that they could reduce considerably the dimensions of their cars, and diminish the proportion of dead weight to live weight; but we hold that, retaining the same gauge, this proportion can never be as small as on narrow-gauge roads. We maintain that if they reduce the dimensions of their cars without reducing in proportion the diameter of their wheels, and this in order not to break their drawhead, they will have each of their cars weighing about 800 or 1,000 lbs. more

than the narrow gauge cars running on smaller wheels and of same capacity. If they reduce the diameter of the wheels and economize to 100 or 125 lbs. per each wheel, they will experience a difficulty just as great as that occasioned by the breaking of gauge. They will break the drawhead of their cars. Some of their cars running on 33 in. wheels will have their drawhead at 33 in. above the rails; the others running on 24 in. wheels will have their drawhead at 24 in. above the rails. Difference between the drawheads 9 in. The cars could only be coupled by heavy bars of flat iron like those used for coupling the tender of the locomotive to the train. Any upset ordinary link of round iron would not do. It would stretch or break. The use of such coupling bars would be unhandy and expensive.

We also maintain that, diminishing or not the diameter of their wheels, they will find another difficulty: that of the liability of the new small cars to be crushed by the old ones, which are heavier and have more momentum.

These two difficulties: the breaking of the drawhead and the liability of the light cars to be crushed by the heavier ones, are the two principal reasons which prevent managers of broad-gauge roads from building a lighter stock. If it is not so, we will ask them why they do not do it, when they might make such a great saving of money by it.

There is also a reason why new companies with a 4 ft. 8½ in. gauge do not build a lighter stock. They fear to let light cars go on others' roads to be mixed up with heavy cars, for fear they would be crushed; the heavy cars would override them in a sudden halt or collision.

The difficulties in the way of using lighter rolling stock on wide-gauge roads, as mentioned above, are so great that they have been considered by several newly organized companies as worse than breaking the gauge, and these companies have preferred to avail themselves entirely of the great saving which exists in having cars of such small weight and such great capacity as those running on narrow-gauge roads; and, feeling that they could not with safety let their light cars go on broad-gauge roads, or allow the heavy broad-gauge cars to come on their roads, they judiciously adopted a narrow gauge, and, building all their stock uniform for that gauge, they averted the difficulties above mentioned. By running their light cars on smaller wheels, 24 in. instead of 33, they saw that the centre of gravity of those light cars was lower than that of the broad-gauge cars, and that consequently they could reduce the gauge in proportion and run their cars with the same angle of stability as the broad-gauge cars.

The reduction of gauge, I maintain, is a natural consequence of the diminution of the dimensions of cars and of their wheels. The lowering of the centre of gravity allows the reduction of the gauge and gives an economy per car in the reduction of weight of axles and break beam, which I estimate at 80 lbs.

There may be on the New Jersey Central one or two cars of reduced dimensions, but is there in all the United States a railroad with a 4 ft. 8½ in. gauge with all its stock of cars as light in regard to their capacity as those run on the Denver and Rio Grande? I think not. Why do not any of those newly built wide-gauge roads run a light stock on 24-in. wheels? Is it not clearly for the reasons given above? So that the broad-gauge companies are compelled to keep their costly stock built under such unfavorable conditions as regards the proportion of dead weight to live weight. The best proof of this is, they do not alter it, and that even in renewing their stock there is no tendency to reform in that respect, as far as I know.

New companies who wish to economize as much as possible in building their road and stock, and fuel, prefer to adopt the narrow gauge. By so doing, they avail themselves of all the economy possible in dead weight.

The only inconvenience they experience is the breaking of bulk, but they know that the immense saving they effect in adopting the narrow gauge and corresponding stock, compensates a hundred times the inconvenience and expense of changing cars and breaking bulk. I believe, like my opponent, Mr. S., that the great damage and inconvenience growing out of an interchange of cars upon thousands of miles of connecting, and in many instances hostile or competing lines, overbalance any good or real saving to the stockholders that may result from it.

I use here the same expressions as Mr. Seymour, in his pamphlet (page 32), and it is one of the very few points on which I agree with him.

One of the great objections made against narrow gauge is the supposed want of room and comfort in the passenger cars. This objection has no foundation whatever. The passenger cars running on the Denver & Rio Grande are the best answer to it. Those cars are built with three seats across on a line instead of four. On one side of the car, for half of the car, are the double seats and on the other side the single seats. In the other half of the cars the single seats are on the left and the double seats on the right. I do not see why for each of these seats there should not be just as much room given to each passenger as there is in the broad-gauge cars.

I will now show Mr. S. why the rails on narrow-gauge roads may be lighter than those generally used on wide-gauge roads.

If with a box car weighing empty 4,500 lbs. I carry a load of 8,000 lbs., is it not true that to carry a load of 80,000 lbs. it it will take 10 of those narrow-gauge cars, weighing altogether 45,000 lbs.?

Also, if a wide-gauge car (4 ft. 8½ in. gauge) weighing empty 18,000 lbs., has a capacity of only 22,400 lbs., is it not true that to carry 80,000 lbs. it will take 3 and $\frac{57}{100}$ cars, weighing altogether 64,000 lbs.

Can I not consequently carry with the narrow-gauge cars a load of 80,000 lbs. with 19,000 lbs. less dead weight than if using the 4 ft. 8½ in. gauge cars. Let us suppose now a train of 60 cars on the narrow-gauge road. Those 60 box cars will haul 480,000 lbs. of live weight and will weigh 270,000 lbs., and the whole train will weigh 750,000 lbs., or 334.84 tons.

To haul the same load on a 4 ft. 8½ in gauge with the cars now in use on such roads, it would take 21 and $\frac{3}{7}$ cars, weighing altogether 385,704 lbs., so that the whole weight of the train would be 865,704 lbs. or 385.65 tons—a difference of 115,704 lbs. of weight, or 51.65 tons, in favor of the train running on narrow gauge and composed of 60 loaded cars.

Supposing now the resistance to be 10 lbs. per ton at a speed of 12 miles per hour on straight and level line, and the two trains running at that speed. Supposing also that the freight engines used on each gauge have six wheels each; is it not evident that the weight on each of the drivers of the narrow-gauge engine will be less than the weight on the drivers of the wide-gauge engine, and that consequently the rails on the narrow-gauge road can be lighter. The adhesion on rails of the narrow gauge will be 3,348.40 lbs., or 558 lbs. for each driver.

Supposing now the adhesion to be in dry weather 600 lbs. per ton of load on drivers, the load on each driver will have to be $\frac{93}{100}$ ton, or 5.58 tons for the six drivers. Such an engine, if it comes from Baldwin Works, Philadelphia, would weigh 15,000 lbs., having 12,499 lbs., or 5.58 tons on the drivers. But that engine must overcome the resistance caused by the friction of her different parts, a resistance equal to $6.69 \times 15 = 100.35 + 334.84 = 435.19$ lbs., the resistance of friction of the engine being generally considered equal to 15 lbs. per ton of weight of the engines, to which must be added 1 lb. for each ton of the load drawn by it. This resistance of the locomotive itself will require a new load on the driving wheels equal to

$$\frac{435 + 2240}{600} \text{ or } 1624 \text{ lbs.}$$

The total weight on each driver would be then $\frac{93}{100} + \frac{12}{100}$ of a ton or 1.05 ton. Such an engine could run on level and in straight line on a rail weighing 12.6 lbs. per yard.

Let us make now the same calculation for the engine running on the 4 ft. 8½ in. gauge. The total weight of the train being 386.65 tons, the total resistance of the train will be 3,866.5 lbs., or 644.4 for each driver. To cause this adhesion there will be necessitated 1.07 ton of load on each driver, or 6.42 tons on the six drivers. Supposing that the load on the drivers in the 4 ft. 8½ in. gauge engine is in the same proportion to the total weight of the engine as in the narrow-gauge engine; such an engine would weigh 17,257 lbs., and would have a resistance of friction of its parts equal to $15 \times \frac{17257}{32240} + 386{,}65 = 502$ lbs.

This additional resistance will necessitate an additional adhesion, which must be obtained by an additional weight of 1,874 lbs. on the drivers, or 2,249 lbs. to the engine. So that the engine ought to weigh 190,506 lbs. and throw 16,255 lbs. on the

six drivers, or 2,709 lbs. = 1.21 ton on each driver.

By using the same formula, $W = L \times 12$, that we used before, to find the weight of rail per yard corresponding to the narrow-gauge engine (in which W represents the weight of rail per yard, L the load in tons on each driver), we find that such an engine on level, would require a rail of 14½ lbs. per yard.

This gives an economy of 2 lbs. per yard in favor of the narrow-gauge rail, the economy being small in this case, because the comparison has been made between two light trains running on a level, and in straight line.

We have assumed that the engine used on the 4 ft. 8½ in. gauge road would throw, like that of the narrow gauge, 83 per cent. of its weight on the drivers, which is not the case, as an ordinary 6-wheel freight engine on a 4 ft. 8½ in. gauge road, 18-in. cylinder, and 24-in. stroke weighs 66,750 lbs. and throws only 43,750 lbs. on the drivers, or about 65½ per cent. of its weight, instead of 83 per cent., and consequently the 4 ft. 8½ in. gauge engines, being heavier in proportion than the narrow-gauge engines, will have a greater resistance of friction in their parts proportionally, and will require proportionally more weight on their drivers, and consequently a heavier rail.

I will now show that the heavier the grades the greater will be the difference between the weight per yard of the rails on the two roads.

Let us suppose that on both roads the engines have to go up gradients of 48 ft. per mile, and to draw the same live load at the same speed of 12 miles an hour. Of course, both of them must be more powerful, heavier, and with more load on the drivers than for a level line.

It has been found by experience that an engine running at a rate of 12 miles per hour on level and in straight line with a resistance of 10 or 10¼ lbs. per ton, when it has to run at the same speed up a grade of 48 ft. per mile, can only draw at that speed the third part of the load she could draw on a level; or, in other terms, to draw on that grade the same load that it could draw on a level and at the same speed, she ought to be three times as powerful and have three times as much weight on the drivers. The engine on the narrow gauge must therefore have 3.15 tons, and the engine on the wide gauge 3.63 tons on each driver.

The rail for the narrow gauge will have to weigh 37.8 lbs., and the rail for the wide gauge 43.56 lbs. per yard.

The difference being 5.76 lbs. per yard.

It is well to observe, at once, that the rails on a 4 ft. 8½ in. gauge weigh generally from 55 to 60 lbs. a yard, because they draw heavier trains than one weighing 386.5 tons live load included, because the grades are generally in some parts of the roads heavier than 48 ft. a mile and also because the roads instead of being in straight line, as I supposed it in my comparison, have curves, and also are exposed to strong gales acting sometimes in front, sometimes on the bulk of the trains by their sides, all causes necessitating more powerful and heavier engines and also heavier rails. The greater this resistance the greater must be the disproportion in weight of rails between the wide and the narrow gauges.

On the other hand, the rails on a 3 ft. gauge are lighter than 37.8 lbs., generally only 30 lbs a yard, because on those roads it is not customary to haul more than 184 tons instead of 334 tons under the supposed conditions.

I have in my comparisons neglected the resistance of the curves on both gauges, and have not taken advantage of the diminution of frontage in the narrow-gauge cars in determining the resistance of the air. The resistance, 10 lbs. per ton, includes the friction of the cars, the resistance of the air front and side of train and the resistance due to concussion and imperfection of road. The resistance caused by friction of cars being estimated at 6 lbs. per ton of weight of train, the resistance of air at $\frac{1}{400}$ lb. per sq. ft. of frontage and for a velocity of 1 mile, and the resistance caused by concussion and imperfection of road at ¼ lb. for each ton of the train at a velocity of 1 mile per hour; all these resistances having been carefully and exactly determined by the experiments made by Mr. Scott Russell.

In the example above given the engine for narrow gauge throwing 18.9 tons on its six drivers and weighing 22.7 tons, would be accompanied by a tender of 1,600 gallons, weighing 13,000 lbs. when empty and 32,000 when loaded. I have for this the authority of Messrs. Baird & Co., who are building narrow-gauge en-

gines at Philadelphia. This makes the total weight of engine and tender when empty equal to 63,848 lbs. and equal to 82,848 lbs. when the tender is loaded.

The engine for 4 ft. 8½ in. gauge, which throws 21.8 tons on its drivers and would weigh 26.2 tons, would be accompanied by a tender weighing 34,000 lbs. when loaded, and of a capacity of 1,800 gallons, so that the total weight of the engine and tender would be 92,688 lbs., making a difference of 9,840 lbs. of dead weight in favor of the narrow gauge.

It is well to remark here that although the Baldwin engines are built in very good conditions, throwing 83 per cent. of their weight on the drivers, still they require, to do the same work, a heavier rail than would a Fairlie engine, and I will seize this opportunity to call attention to the advantages, in my opinion, of that new system of locomotives, the Fairlie engine. It is, in my opinion, the one which gives the greatest tractive power and adhesion with the minimum dead weight of engine.

There have been no locomotives of another system built yet, that I know of, weighing only 60 tons, tank included, and having a tractive power of 35,550 lbs., or 26 per cent. of the dead weight. The Baldwin locomotives, weighing 41,000 lbs. with their tank empty, have only a tractive power of 7,300 lbs., or 18 per cent. of the dead weight of engine and tender.

I have also the proofs that the Fairlie engines perform well and are more economical than any other kind of engines as regards consumption of fuel. They have the enormous advantage of throwing their whole weight on the drivers; on a 36 lbs. rail put on a 3 ft. or 3 ft. 6 in. gauge road, you can, with a Fairlie freight engine of 12 wheels weighing 36 tons, have the same tractive power that would be given by 2 ordinary freight engines of 6 wheels each, weighing each 27½ tons and throwing 43,750 lbs on the drivers. The economy of dead weight being, therefore, 42,456 lbs. in favor of the Fairlie engine, which could draw on a 36 lbs. rail on level, the same load that ordinary engines could not draw with safety without running on a 39 lbs. rail, the distance between the centres of the ties being in each case 2 ft.

Consequently, in the above comparison I made, if a Fairlie engine was employed on the narrow gauge and ordinary engines on the 4 ft. 8½ in. gauge, the weight of the rails on the narrow gauge might be only 34.8 lbs. per yard instead of 37.8 lbs., as we found before, while those on the wide gauge ought to weigh 43.56 lbs. per yard, giving a difference of 8.76 lbs. per yard in favor of the narrow gauge when a Fairlie engine is used.

In examining the proportion of weight on the drivers to the whole weight of the engine in those cited by Mr. S. (p. 29 of his pamphlet). I see that an engine of a total weight of 66,050 lbs. has only 40,050 lbs. or 60 per cent. of its weight on the drivers; that another on the Lehigh Valley Railroad, a 10-wheeled engine of a weight of 78,000 lbs. has only 63,000 lbs. or 80 per cent. of its weight on the drivers, and I remark that the weight of the tenders has not been given with those of the engines. The one of the New York & Erie, weighing 66,050 lbs., must be accompanied by a tank weighing at least 42,000 lbs., so that it throws on the drivers only 37 per cent. of the total weight of engine and tender. The 10-wheeled engines of the Lehigh Valley Railroad, weighing 78,000 lbs., must have tenders weighing at least 36,000 lbs., so that they throw on the drivers only 55 per cent. of the total weight. The Consolidation engines, weighing 86,000 lbs., must have tenders weighing at least 36,000 lbs., so that they throw on the drivers 62 per cent. only of the total weight. None of them, like the Fairlie engine, throws the total weight of engine and tank on the drivers, and I will call the attention of Mr. S. to the fact that the broad-gauge engine of the New York & Erie road, is, in that respect, in the worst condition.

I cannot understand why the Fairlie engines are not getting popular in this country. The engines built by Messrs. Baird & Co. throw on their drivers only 59½ per cent. of the total weight of the engine and tender. It is considerably more than the broad-gauge engines can do, but it is very far yet from what the Fairlie engine does. These latter are like all new things; however good they may be, they meet at first only incredulity and opposition.

Mr. William Mason, of Taunton, Massachusetts, has built one weighing, in working order, with its tanks and coal bunkers full, from 60 to 70 tons, and he says: "Notwithstanding the great weight, the

machine runs so smoothly on the track that I am quite certain that it is not as destructive to the rails as an ordinary 8-wheeled engine. It will go around a curve of 150 ft. radius with as much ease as a 12-wheeled car, and it will haul the load of 4 ordinary 8-wheeled machines of the same-sized cylinder."

The generality of the American builders criticise them and try to create the impression that they are not economical, but we know from engineers and managers of roads where they have been at work, that not only are they economical but are more so than the generality of engines now in use. We give here a copy of a letter addressed to Mr. R. F. Fairlie by Mr. A. McDonnell, Chief-engineer of the Great Southern and Western Railroad of Ireland, who is using the Fairlie locomotive on his line :

GREAT SOUTHERN AND WESTERN RAILWAY,
LOCOMOTIVE ENGINEER'S OFFICE,

July 19th, 1870, Inchicore, Dublin.

R. F. FAIRLIE, Esq.,

DEAR SIR,—In reply to your inquiries as to the two double bogie engines built in these works on your system, one has run 5,677 miles, and the other 3,867 miles. They both worked as pilot engines, when turned out new, and afterwards as passenger engines with slow trains ; the average running speed of the trains they work is from 25 to 30 miles an hour, and the load from 4 to 9 of our large 6-wheeled carriages. The weight on the wheels is well distributed, being 10 tons on each pair of wheels of the steam bogie, and 8 tons on each pair of wheels of the trailing bogie in working order, with water enough to run from 20 to 25 miles, and coal enough for the run from Dublin to Cork (165 miles.

The engines make steam extremely well and the consnmption of coal is low. The drivers consider its 3 or 3 lbs. a mile less than the average. I will, however, let you know more exactly soon, when they have worked longer as train engines.

The engines work very freely, run very steadily, and are very handy.

They have given no trouble except a little with the steam pipes, which I have altered since. They will run round curves of 100 ft. radius, and run freely round curves of 300 ft. radius, which is all I require them to do in practice.

Yours truly,

A. MCDONNEL.

I will add to this that General Buell has received a letter from Mr. B. Saloff, Chairman of the Board of Management of Railroads in Russia, giving him some details about the construction of narrow-gauge roads and the performance of the Fairlie engines in Russia. Here are extracts of that letter : " These engines take a train of 61 loaded wagons, gross load 410 tons, the length of the train exclusive of engine being 896 ft., at the rate of 10 miles an hour up grades of 1 to 80 or 66 ft. a mile, and consume only 3.03 cubic ft. of wood per mile. There is room on the engine for 320 cubic ft. of wood, 198 cubic ft. of water in the tanks, and 130 cubic ft. of water in the boiler." That letter was written to General Buell by order of Count Alexis Bobrinsky, Minister of Public Works in Russia, in answer to one written to him by Gen. Buell and asking information about the narrow-gauge roads in Russia.

Mr. S., who writes (page 29 of his pamphlet) that for the performance of engines on narrow-gauge railroads, he can only refer to theoretical and assumed results, can see that we have something better to offer. We present facts with the authority of the Minister of Public Works in Russia. The Fairlie engines are working there on the Livny railway (3 ft. 6 in. gauge.) The rails weigh 45 lbs. to the yard on account of the heavy trains hauled and of the heavy grades. If the gauge was 4 ft. 8½ in., and the rolling stock and engines similar to those used now on such gauge roads, they could not weigh less than 60 lbs. to the yard.

The original of Mr. B. Saloff's letter is in the Archives of the Texas Pacific Railroad Company. We hope that the results of the performance of the engines of Messrs. Baird & Co., Baldwin Locomotive Works, on the Denver and Rio Grande and other narrow-gauge roads, will soon be published. Now, it is evident that the Fairlie engine would perform also very well on a broad gauge; not as well as on a narrow one, however, because the narrow gauge admits of sharper curves than the broad gauge, and it is especially in the facility of turning in the sharp curves

that the advantages of the Fairlie engines are found. The greater number of their driving wheels and the equal distribution of the weight on them allows also the use of a lighter rail. These engines perform economically their work with the least dead weight possible. Mr. Fairlie, however, is mistaken when he allows himself to say that a narrow-gauge road without his engine is a delusion and a snare. The absence of Mr. Fairlie's engine from a narrow-gauge road does not reduce to naught the economy of that road in building it, equipping it, and running it. Having in the preceding demonstrated that the rails on a narrow gauge must be necessarily lighter than those on a wide, gauge road, because in the narrow-gauge cars the proportion of dead weight to live weight is less. I have then shown the error of Mr. S. when he says (page 9 of his pamphlet) : "If it is claimed that the same amount of tonnage can be hauled with greater economy by *multiplying the trains* and using *lighter engines*, then I maintain that the same principle can be applied upon the wider gauge with equal economy, and therefore that no greater weight of rails is required." The calculations I have made before and the facts show the fallacy of his assertion.

In my comparison of the two engines in which 48 ft. a mile was the greatest grade on the two roads, and the same regular speed of 12 miles an hour being maintained on that grade, the engine on the 4 ft. 8½ in. gauge road does not weigh more than 26 tons. It is considerably lighter than the engines usually employed for freight on the 4 ft. 8½ in. gauge roads. These engines generally weigh from 30 to 40 tons, the weight of tender not being included. With their tanks they generally weigh 45 to 56 tons. Consequently to haul with that engine weighing only 26 tons the same load that could be hauled with one weighing 35 tons, it will be necessary to multiply the trains. I made my comparison under the circumstances preferred by Mr. S., viz. : The multiplication of trains. But he can multiply them as much as he chooses, he will never, as long as he uses the same cars which are used now on the 4 ft. 8½ in. gauge roads, avoid the contingency which I have demonstrated, that for a live load of 80,000 lbs. to be hauled at a speed of 12 miles an hour, in straight line up a grade of 48 ft. per mile, the rails on a wide-gauge road will have to weigh 43.56 lbs. per yard, while on a 3 ft. gauge road the rails need weigh no more than 37.80 lbs. per yard if an ordinary engine is used, and 34.8 lbs. per yard if a Fairlie engine is used.

As we have said before, we do not pretend that the reduction of the proportion of dead weight in cars cannot be done without reducing the gauge ; we know, on the contrary, that the reduction of gauge is a consequence of the reduction of the dimensions of cars, and consequently of their dead weight, but we maintain that the broad gauge companies are bound to have each of their eight-wheeled cars, weighing 800 or 1,000 lbs. more than those on the narrow gauge, and we maintain that with a stock as light as that of a narrow gauge they lose all the advantages that they claim in keeping their broad gauge, and would find themselves in the same position as if they had broken the gauge, because their light stock does not admit of running with the heavier stock of adjoining roads, or even of being mixed with heavier stock on their own roads, without risk of crushing. That, consequently, broad-gauge railroad companies, without inconvenience amounting almost to impossibility, cannot materially reduce the dimensions of their stock and make it lighter if they want their cars to run on connecting roads, and reciprocally cars of other roads to run on their own. That if they refrain from breaking the gauge for the sake of retaining their cargoes in bulk, they are compelled to keep their deficient stock and cannot bring it in as good condition as regards the proportion of dead weight to live weight as the narrow-gauge cars, without risk of broken drawheads and crushed cars.

From what I have said before, it is evident that the reduction of gauge is a natural consequence of the reduction of the dimensions of the cars, and that the gauge must be proportional to the dimensions of the cars.

The idea of running on a wide-gauge road cars as small as those of the Denver and Rio Grande Railroad, seems to me ridiculous. What would be thought if Mr. Seymour was building a road-bed as large as that of the New York and Erie, with a track of 6 ft. gauge, and was running box cars 14 ft. long, and 6 ft. wide,

and flat cars 12½ ft. long, and 6 ft. wide? Would it not strike every one, that if these same cars are running on the Denver and Rio Grande on a 3 ft. gauge, laid on a road-bed considerably smaller, and built at a considerably less expense, there can be no justification for the extra expense of road-bed and of diameter and length of axles and wheels, etc., etc.? I repeat it again, the gauge must be proportional to the dimensions of the cars, and Mr. S. in putting a flat car 12½ ft. long and 6 ft. wide, and of same dimensions of timber as those of the Denver and Rio Grande, on the 6 ft. gauge of the New York and Erie, would commit as great a blunder as if he was taking the box of a sleeping coach of the New York and Erie to put it on a truck running on a 3 ft. gauge. In the first case there would be too much stability and unjustified expense of road-bed and materials, and in the second case there would not be stability enough, and nobody would risk his life in such a car. The large cars must run on a wide gauge, and the small ones on a narrow gauge. Good common sense endorses this principle. But in case there should be in the mind of Mr. S. or anybody else any doubt about this, we are going to demonstrate it.

To do it, however, we shall have to use formulas and scientific terms. I am really sorry for it, for Mr. S., who thinks that they are not necessary for the discussion of such a question, but, if he is open to conviction, I have to do it to try to convince him. Let us state first that Gen. Buell never had any idea that his report, which was addressed to the President and Executive Board of the Texas Pacific R.R. and was not intended for publicity, would be ever read by farmers, teamsters and carters. Those men (teamsters or carters) who may be very skilful and clever in their line of business, have not, generally, sufficient knowledge to understand the matter we are discussing. Gen. Buell, writing his report for the Directors of the Texas Pacific, thinks, like myself, that it would be inappropriate and ridiculous to use terms like "top heavy," instead of "centre of gravity" in discussing this matter.

Mr. S., who wrote for the public at large, teamsters and carters included, might as well have used in his review the terms "Ouhah" and "Dje" instead of right or left, and it would have been, perhaps, still more comprehensible for the teamsters and carters, and more to their taste. I have no doubt, however, that if they saw the sleeping coach of the New York & Erie running on 24 in. wheels on the Denver & Rio Grande they would think it "top heavy," and if they saw a platform of 12½ ft. running on the New York & Erie they would find it "top light."

But let us come to our demonstration. The general formula for the distance of the centre of gravity of a body from a plane is

$$P\,X = p'\,x' + p''\,x'' + p'''\,x'''$$

which gives

$$X = \frac{p'\,x' + p''\,x'' + p'''\,x''' +}{P}$$

in which P is the total weight of the body, X, the distance of the centre of gravity from the plane, p', p'', p''', the weights of the different parts composing the body, and x', x'', x''', the respective distances of the centres of gravity from the plane.

Let us now suppose the body to be a car, and for greater simplification, let us admit that the different parts forming this car can have their weight concentrated at four different points when the car is empty and five different points when loaded.

Those points being, viz.:

1*st*. The trucks including all that is below the sills of the cars, save the bunters and cross-beams resting on trucks, and such as wheels, axles, bolsters, brakes, brake beams, iron rods, etc., all the weight of which can be approximatively considered as acting at the centre of the wheels.

2*d*. The weight of the sills, flooring, crossbeams, bunters and platforms, which we will consider as acting at the centre of gravity of the sills.

3*d*. The weight of the box (roof not included), which we will suppose to act at the middle of the height of the box.

4*th*. The weight of the roof acting at the centre of gravity of the roof.

5*th*. The weight of the live load acting at the centre of gravity of the volume occupied by that load at the top of the flooring.

Let us also suppose the plane to be the horizontal plane of the top of the rails,

p^1 will represent the weight of the trucks.

p^2 the weight of the sills, crossbeams, platforms, flooring and bunters.

p^3 the weight of the box, flooring and roof excluded.

p^4 the weight of the roof.

p^5 the weight of the load.

We can see from the formula that the centre of gravity of the car when empty is situated at a distance from the rails where the total weight of the car multiplied by that distance, equals the sum of the products of the four different partial weights of the car multiplied by the respective distances of their centre of gravity from the given plane.

Now it will be found, on experimenting on the formula, that

1*st*. If without changing any of the other weights or their respective distances from the rails, we diminish one or all the weights acting above the horizontal line in which is situated the centre of gravity, we will lower the centre of gravity.

2*d*. If we diminish the distance to the rails of one or all the weights acting either above or below the horizontal line of the centre of gravity, without changing the other distances or weights, we will lower the centre of gravity.

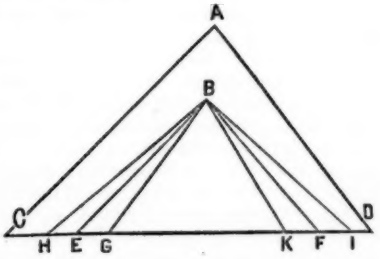
3*d*. If we diminish one or all the weights acting below the line of the centre of gravity, the other weights and distances remaining unchanged, we will raise the centre of gravity.

What are we doing, then, when we reduce the dimensions of a car as much as Jackson & Sharp and others are doing? We certainly diminish all the weights above spoken of and their respective distances from the rails, and consequently we lower the centre of gravity.

If we were reducing the weight of the trucks without diminishing the diameter of the wheels, we might perhaps keep the centre of gravity at the same height or near it, but we diminish the diameter of the wheels also, and the centre of gravity is then necessarily lowered; they run on 33 in. wheels, whatever may be the gauge, and box freight cars for example can be run on 24 in. wheels, and even 20 in. wheels, with considerably less resistance to the tractive power, than the heavy cars offer when they run on 33 in. wheels, whatever may be the gauge, and we call the attention of Mr. S. to this fact.

The formula above cited is the one which has been used by Gen. Buell and myself to calculate the position of the centre of gravity of the cars whose plans are shown in his report, and we know it to be the right one.

Having proved that the centre of gravity of one of the narrow gauge cars above cited, even if the wheels were apart enough to run on a 4 ft. 8½ in. gauge, is lower than that of the cars now in use on the wide gauge, I will prove that the gauge must be reduced in proportion, and that the dimensions to which it must be reduced to allow the narrow-gauge cars to run with the same stability, as regards oscillations and concussions, as the common cars run on the 4 ft. 8½ in. gauge, can be fixed exactly.



A being the position of the centre of gravity in the 4ft. 8½ in. gauge cars now in use, and C D representing the distance between the centres of the rails or about 4 ft 10½ in., if B is the position of the centre of gravity of the cars of reduced dimensions, we have only from B to draw two lines B E and B F parallels to A C and A D, and the line E F will represent the distance between the centres of the rails of the gauge on which the small cars could run with the same angle of stability as the large ones.

All lines drawn from B, such as B H and B I, and striking the line C D in points H and I, outside of the points E and F, will fix on the line C D, the position of rails on which the small cars could run with a greater stability than the large ones. All lines, such as B G and B K, striking the line C D in points G and K, inside of E F, will fix the position of rails on which the small cars would run with less stability than the large ones.

For fixing the reduction of the gauge, when the reduction in dimensions, weight, and capacity of cars has been made, it is therefore important to know the position

of the centre of gravity of both large cars and small cars.

As regards the stability of the narrow-gauge cars on the road, Messrs. Jackson and Sharp announce that these cars have been drawn at a speed of 40 miles per hour by locomotives (built at the celebrated Baldwin Works of Philadelphia), and have encountered gales, whose severity is too well known to travellers in the Colorado Uplands; in fact no effort has been spared to put them to the severest test. Their success under such circumstances fully insure their future performance, and demonstrates that the gloomy doubts entertained by many minds, respecting narrow-gauge rolling stock were utterly without foundation, and worthy only of a past age.

The saving of dead weight per passenger is about 300 lbs. An equal degree of speed is attainable and equal safety; and from the shortness of the axles, the wheels slip less on the outer sides of curves. This diminishes the correspondent twisting force applied to the axles, which is known as the "torsion strain," which, as is well known, destroys the fibre of the iron, makes all car axles useless after a time, and causes numerous railway accidents. Let Mr. Post, who made that subtle but erroneous and merely theoretical demonstration on curves at the end of Mr. S.'s pamphlet, bear that in mind, and answer the practical facts announced by Messrs. Jackson and Sharp. General W. S. Rosecrans, in a letter on the subject of narrow-gauge railroads, goes into some interesting calculations, which show that if the railroads in the United States, down to the end of 1867, had been built on the narrow-gauge plan, the saving in first cost would have been $480,000,000; the annual interest of which is, at 6 per cent., $28,800,000; that the annual saving on haulage would be $100,800,000; in all, an annual saving to the people of the United States of $129,600,000.

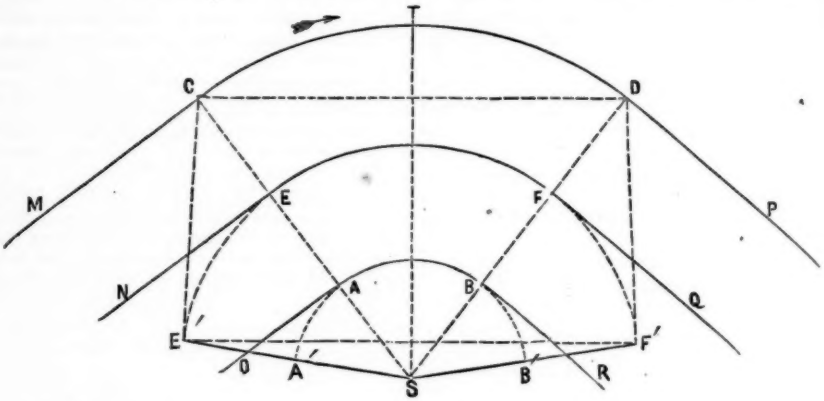
He shows further, that a much greater saving is in question for the future; for, he reasons, provided the whole country is to be as well furnished with railroads as the State of Ohio, there will be a total length of 165,800 miles of railroads; and the annual saving on this length of roads on the narrow-gauge system would be $547,540,515; which would pay off our present national debt in about five years.

Moreover, he shows that railroads, on the average, add $10 an acre to the value of the lands within 10 miles of them; the narrow-gauge roads can be afforded in districts where broad-gauge roads could not, to an extent which it is moderate to call 30,000 miles, which would add to the value of the land bordering these roads, $3,800,000.

Let us say now a word about the demonstration of Mr. S. S. Post in relation to the comparative resistance upon railway curves of different gauges annexed to Mr. S.'s pamphlet. Just at the very beginning the author makes a mistake in supposing that the width necessary between the two tracks of a narrow-gauge is the same as is necessary between the two tracks of a broad-gauge road. This would be true if the narrow-gauge cars were as wide as the broad-gauge cars, but as they are narrower, it follows that the width between the two tracks on a narrow-gauge can be reduced by just the difference existing between the width of the two systems of cars—the space left between two cars opposite, one on each track, being the same on the narrow-gauge as on the broad-gauge road. This is one of the principal advantages of a narrow-gauge when a double track is laid. To the economy of the single road-bed as compared with the wide-gauge single road-bed, we add the economy of saving in the total width of the double road-bed a quantity equal to the difference between the width of the narrow-gauge cars and the broad-gauge cars. This is what Mr. S. and Mr. P. both overlook. But the demonstration of Mr. P. is false from beginning to end. In it he supposes the narrow-gauge car to be of same length as the broad-gauge car, which is a misrepresentation.

To make a fair comparison, it ought to be supposed that the forward axle E A of the forward truck of a narrow-gauge car in leaving the straight track N E, O A, to enter the curve E F A B in the position E A, and after having run through the curve, is in the position B F, ready to take the straight track F Q, B R, and also that the forward axle of the forward truck of the wide-gauge car passes from the position C A to the position B D, ready to take the straight track D P, B R. Mr. Post in a very unfair and erroneous way, assumes that the forward axle of the forward

truck of the narrow-gauge car has, in order to pass from one tangent to the other, or from one straight track N E O A, to the other straight track F Q B R, and through the curve A B E F, to start from the position E′ A′, and stop in the position B′ F′, the points E′ and F′ being determined by the intersection of the two parallels C E′ and D F′ with the curve of the outer rail of the narrow-gauge track,



and this on the wrong hypothesis, that the length of the narrow-gauge car must necessarily be the same as that of the broad-gauge cars.

The inspection of the figure and the positions of the points E′A′ and B′F′, which ought to be on the straight line and are out of it in an impossible position, shows the fallacy of Mr. Post's demonstration.

Now, I claim that the wheels will have to slip more from C to D than from E to F, and I claim also that the friction when the wheels slip is considerably greater than when they are rolling. That consequently on the outer rail C D of the broad gauge the wheels will have a greater friction and for a longer distance than on the outside rail E F of the narrow gauge, and that consequently the friction or resistance on the narrow gauge will be less than on the broad gauge, and as I have said before, the torsion strain, which is so injurious to the axles, will be less on the axles of the narrow-gauge car than on those of the broad-gauge car.*

The above formulas, which are from standard authors, testify the least resistances are experienced by the narrow gauge.

Having disposed of the demonstration of Mr. Post, as I have, I think, disposed of the arguments of Mr. S, and shown

*The curving of a railroad causes three different kinds of sliding friction.

The first is caused by the fixity of the wheels on the shaft. One of the wheels on the outer rail slides a distance equal to the difference of length of the two curves. The second is caused by the parallelism of the shafts, causing the wagon to slide on the rails, turning round its centre of gravity while changing direction.

The third is caused by the centrifugal force, which forces the flange of the outer wheels to be in contact with the outer rail. Of the second kind of sliding friction, which is very important in European countries, we will not speak, because in America it is prevented by using centre pins under the wagons and engine tenders.

Theoretically the centrifugal force is less than the resistance due to the friction of the wagons on the rails for ordinary speed, and for a radius of 500 yards the flanges of the wheels ought not to be much in contact with the rails, if the wagons were not jumping; but as this effect is always produced, the friction due to centrifugal force is for wagons expressed by the formula

$$f'' \frac{P+p}{g} \times \frac{V^2}{r} \frac{2c}{D}$$ in which

V = speed of the centre of gravity of the wagon.
D = diameter of wheel.
c = horizontal distance from the vertical passing through the centre of the wheel to the point where the flange begins to touch the rail.
f'' coefficient of sliding friction of the flange of the wheel, found to be equal to 0.25.
g = acceleration due to gravity.
$P+p$ weight of wagon.
r radius of the circumference described by the centre of gravity of the wagon.

The friction of the first kind mentioned, caused

their fallacy, I will now say a word about the 3 ft. 6 in. gauge that General Buell and myself recommended to the Texas Pacific Railroad.

Let me state first that I consider that it would be much better for the Texas Pacific to have a 3 ft. gauge than to have one of 4 ft. 8½ in. But General Buell and myself think that 6 in. more added to the gauge would allow to give still more room and comfort to passengers, and would allow a better shipment of cotton and cattle. If, however, the question was between the 4 ft. 8½ in. and the 3 ft., I would say take the 3 ft.

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by the fixity of the wheels on the shaft is equal to $f\frac{2a}{r}$ in which

a = half width of track.

r = radius of the circle followed by the centre of gravity of the wagon.

f = coefficient of friction of the wheels on the rails, equal to 0.25.

For a wagon, the resistance is $f(P+p)\frac{a}{r}$.

(P + p representing total w i ,ht of the wagon.

From this formula it can be seen that the greater radius of the curve the less is that friction; also the smaller the gauge the less the friction.

Now there is a fourth kind of friction, which is due to the rolling of the wheels on the rails.

That resistance is equal to $R=f'(P+p)$ in which

R = resistance.

P + p = total weight of the wagon.

P being the total weight of the wagon on the wheels, and p the weight of wheels and shafts.

f' = coefficient of friction, equal to 0.001 (one thousandth) for wheels of 33 inches diameter.

Consequently the coefficient of rolling friction is only 0.001 while that of the sliding friction is 0.25.

CURRENT OBSERVATIONS.

By Geo. H. Mann, C. E.

Written for Van Nostrand's Magazine.

The general purpose of current observations is the determination of the velocity and direction of flow of running water in connection with such special purpose as the gauging of the discharge of rivers, the location of bridges, the ascertaining of the influence of docks, piers, and dykes upon the flow of bodies of water, the projecting of plans for the improvement of rivers and harbors, and the development of systems of torpedo defence. In considering them, two classes may be properly distinguished.

1. For the determination of the velocity alone.

2. For the velocity and direction of flow.

The application of the first class is more especially the gauging of discharge, and as any of the methods under the second head can also be used for this purpose, those which are applicable to velocity alone will be first considered.

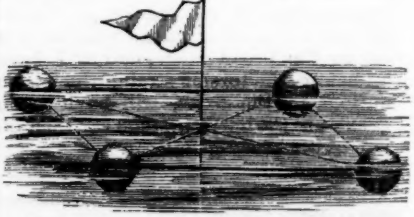
In computing discharge it is necessary to ascertain the mean velocity in a given cross section, and although this can be calculated from the surface velocity, it is generally necessary to determine velocities at the surface, at the bottom, and at intermediate points. For these, any of the following methods may be used.

1. By Floats.
2. By Woltmann's Mill.
3. By Pitot's Tube.
4. Hydrometric Pendulum.
5. By Impact Hydrometres.

For ascertaining the surface velocity, the simplest method is to note the time of passage of any light floating body, as a wooden ball between any two ranges at a known short distance apart; then

$$v = \frac{\text{distance}}{\text{time}} = \frac{d}{t} \cdot a.$$

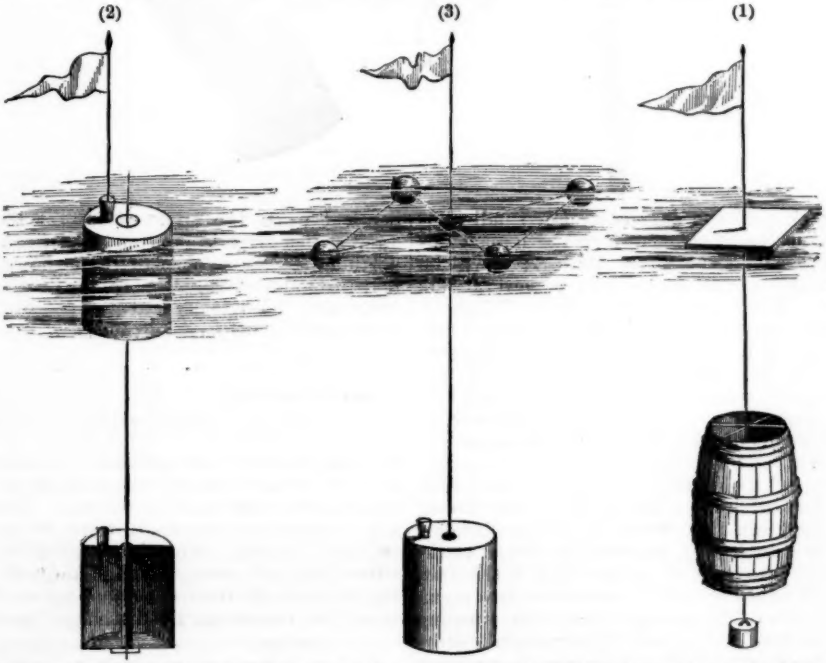
A good form of float, but little affected by the wind, and readily visible, is shown in the diagram. It consists of 4 hollow



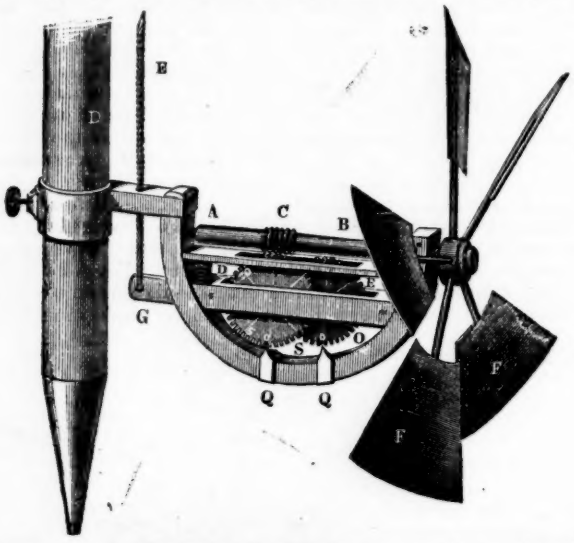
rubber balls, united by a very light frame, supporting a thin rod and a flag, and so weighted as to almost submerge the balls. Brass balls from 4 to 12 in. diameter, and

painted with light colored paint, may also be employed. By uniting 2 bodies, presenting equal surfaces to the current, by a thin wire, the velocity at various depths may be ascertained, one ball or body being weighted to sink and the other so as to allow the least portion possible projecting above the surface. Then, if we ascertain the surface velocity *bs*, simultaneously by a single float and denote the velocity obtained by using 2 floats by *vm*, we have for velocity at depth of second ball $bd=2vm-vs$. By placing the lower ball at different depths we obtain the velocities corresponding to those depths, from a sufficient number of which we may determine the mean velocity. This can also be found by employing a floating staff weighted so as to float perpendicularly; it is conveniently made in short pieces which can be screwed together to adapt its length to the depth. The velocity at any particular depth is determined by having the submerged body present so large a surface that in comparison with it that of the surface float shall be inconsiderable, then by allowing the float to start some distance above the first range, so as to gain headway, we find the velocity at the depth of the submerged body. The forms of floats employed are so numerous that an attempt to describe all of them would occupy more space than the limits of this article will admit of. A number of them are shown in the diagram.

In (1) the lower float is a barrel with both heads out, weighted so as to float upright; (2) and (3) are very convenient forms, and consist of tin cylinders having a tube running down the axis so that the wire can be readily lengthened or shortened; a small orifice closed with a plug allows the admission of water for submerging them. The surface float of (3) while offering but little surface to the action of the current, has a broad base and is not readily overturned. For observations in salt water, copper instead of tin floats are preferable, as those made of tin corrode very rapidly. Wire should generally be employed to connect the floats, rope being apt to chafe off. For small depths, strong cord answers very well.



For the direct determination of the velocity at any point, the best form of hydrometer is Wollman's Mill or Tachometer, shown in the diagram. The instrument is attached to a pole so that it can be immersed to any depth. It consists of a frame supporting a shaft A B, having attached to it either a small propeller screw or from 2 to 5 vanes inclined to the direction of the axis at angles varying somewhat with the velocity of the current (a vane serves to keep it in the direction of the current). For small velocities this angle, equal to the impact angle, should be about 70 deg.; it will be found advantageous to have several sets of vanes for varying velocities. When we immerse the mill in running water, and hold it in a direction opposed to the current, the number of revolutions of the shaft depends upon the velocity, and this measures it. By having a number of threads of an endless screw cut upon the shaft and working into the teeth of a cog-wheel D, we can, by means of an indicator and figures marked upon this wheel, ascertain the number of its revolutions, and by having another cog-wheel working into a pinion on the shaft of the first, a larger number of revolutions are registered. The gearing is kept from turning except during time of observation by having its shafts run in bearings upon a lever G, which is pressed down by a spring so that it is only by drawing the string E, upward that the teeth of the first cog-wheel take into the endless screw. In consequence of resistances, and from the fact that the number of revolutions is dependent upon the angle of impact, the number in a given time is not exactly proportional to the velocity of the water, so that instead of having $V = An$, in which n = number, V = velocity and a = a constant determined by experiment, we have



$$V = V^0 + A\,n \quad . \quad . \quad . \quad (1.)$$

or more accurately

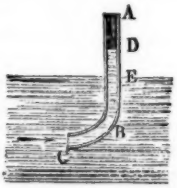
$$V = A\,n + \sqrt{V_0^2 + C n^2 V_0}$$

denoting velocity just sufficient to start the mill, A and C empirical constants determined by experiment, as follows: The mill is placed in a current of water, whose velocity is known (either by observations with a float or by using a trough and making the quantity discharged and received equal, and measuring the discharge)* and

*In order to determine the velocity by measuring the water discharged from a trough under a constant head it is only necessary to get in addition the dimensions of the cross section at point of immersion; then we have $V = \frac{Q}{F\,t}$, in which Q = quantity, F area of cross section, and t = time. From these observations the constants may be

the number of revolutions in a given time is noted, as many observations as possible being taken.

Pitot's Tube is a simple form of hydrometer, and consists of a bent tube, generally of glass, held so that its lower branch is horizontal, and opposite to the direction of the current. The column of water within the tube is forced up by the impulse of the current above the level of the exterior water; the rise is dependent upon the velocity of the water, and may be



used to determine it. If h = rise and c = empirical constant determined by experiment, then $v = c\sqrt{h}$. To find c we place the tube in water of known velocity v, then $c = v\sqrt{h_1}$. $\therefore c = \frac{V^1}{\sqrt{h^1}}$. For convenience in reading the height h, the instrument generally consists of 2 tubes, from one of which a pipe proceeds contrary to the direction of the current, and from the other, 2 pipes at right angles to it, then by closing both tubes at once and drawing hydrometer of the water the difference of heights can be easily read. The oscillations in the tubes may be obviated by making their mouths narrow. For velocities ranging from 1 to 4 ft., by using good instruments it has been found that $V = \sqrt{h} \times 11.5$ very nearly. Pitot's tube is an easy instrument to use, and under favorable circumstances gives very good results.

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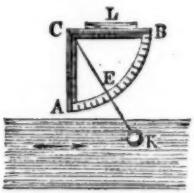
deduced by introducing values of V and n into (1) or (2) and forming equations of condition from which A and C V_0 can be found by combination, or using (1) we have by method of least squares

$$V_0 = \frac{\Sigma(D^2)\,\Sigma(c) - \Sigma(b\,c)\,\Sigma(b)}{\Sigma(c^2)\,\Sigma(b^2) - [\Sigma(b)]^2}$$

$$A = \frac{\Sigma(c^2)\,\Sigma(b) - \Sigma(b\,c)\,\Sigma(c)}{\Sigma(c^2)\,\Sigma(b^2) - [\Sigma\,b\,c)]^2}$$

in which $b = \frac{n}{v}$ and $c = \frac{1}{v}\,\Sigma$, denoting sum of all the values. It will of course be necessary to determine constants for each different vane wheel.

Hydrometric Pendulum.—This hydrometer consists of a graduated quadrant (see diagram) having attached to its centre a string or wire to which is fastened an ivory or metal ball. The angle between the vertical and the string when the ball is held in running water, depends upon the velocity. For the purpose of placing the zero line vertical, a level tube is put upon the instrument. An ivory ball is used for velocities less than 4 ft. and metal ones for greater velocities. If

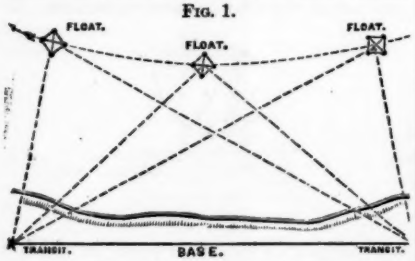


α = angle, we have $b = c\sqrt{\tan\alpha}$ in which c is an empirical constant determined thus: v = known velocity, α = corresponding angle; then $c = \sqrt{\tan\alpha_1}$. In addition to the hydrometer already mentioned, there are a number of others by which the velocity is ascertained. By measuring by a balance the force of impact upon a surface, the general equation for the velocity is $v = c\sqrt{I}$ in which I = impulse and c = constant dependent upon area of surface. There may be mentioned more as a curiosity than anything else, Leslie's hydrometer, by which the velocity is ascertained by observing the relative rates of cooling of the bulb of a delicate thermometer. We can also determine velocity by immersing a box with a small hole in it, allowing the water to flow in for a certain time, closing it suddenly and comparing with the quantity admitted in the same time in a stream of known velocity. We may determine velocity from the formula $V = \frac{Q}{C}\sqrt{\frac{1}{h}}$, in which Q = quantity, C = empirical constant determined by experiment as before. Thus $C = \frac{Q_1}{V^1 + \sqrt{2gh}}$, in which g = acceleration due to gravity, and h = depth of orifice below surface, C being dependent on the co-efficient of discharge and upon the connection of the orifice.

2. *For determining velocity and direction*, either free or restrained floats (logs) may be used. The various forms of free floats having been described in the pre-

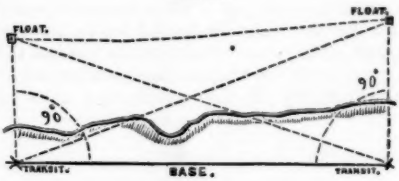
vious section, the methods of using them will now be given. If the observations are to be made where the floats are readily visible from the shore, a base or bases of convenient length should be measured as nearly as possible parallel to approximate direction of the current, and an observer with a transit located at each end, the float being allowed to drop down with the current, its positions are located by measuring simultaneously at each station

FIG. 1.



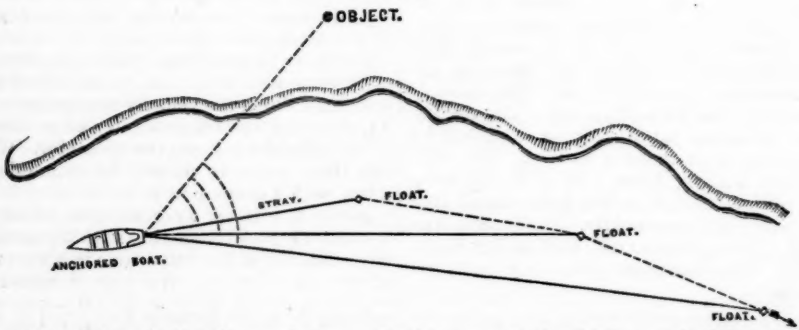
angles between it and the other station; these angles may be taken at equal intervals of time, or the time and angle between the base and float may be taken by each observer when the float crosses a line perpendicular to the base at the other station (see Fig. 1). When the current is rapid the latter (Fig. 2) method may be advantageously employed; the velocity being determined from plotted positions of floats $b = \frac{\Sigma D}{\Sigma t} D$ = distance, t = time.

FIG. 2.



When a base on land cannot be used 2 boats may be anchored to form a base and the positions of the float located by observations with 2 sextants, the position of the boats being determined by measuring angles from each to 3 known points on the shore, or by angles measured with a transit ashore; the bearing of the base and its length measured as nearly as possible by a tightly stretched cod line being taken as additional data. Should it be necessary to take observations when no land is visible, the position of one boat

FIG. 3.



must be determined astronomically, and the relative place of the other found, although in such a case restrained floats or logs are generally employed. Another method is to start the float from some known point marked by a buoy and follow it with a boat, locating its position at equal intervals of time by sextant observations upon 3 points ashore. In using *restrained floats* the boat is anchored and located with respect to the shore, and as the station is likely to be occupied more than once it should be marked by a barrel-buoy whose anchor warp has but little slack. In all observations at equal intervals of time a sand-glass should be employed, especially if the interval is small. To an ordinary ship's log, or preferably to any of the forms of floats given in a previous section, is attached an ordinary log line passing to a reel in the boats. The line, before using, should be wetted and stretched. It should then be marked off into miles and fractions of a mile, as fol-

lows: If we use a 30-sec. glass, a velocity of 1 statute mile per hour will run out the line

$$\frac{3600^s}{30} \times 5280 = 44 \text{ ft.}$$

or of one nautical mile,

$$\frac{3600^s}{30^s} \times 6087 = 50.72 \text{ ft.}$$

These divisions of the line are best marked by knots made in white cord and fastened to it, or thus: 0 is marked by a small strip of leather—1 mile by a strip with a hole in it and so on, making a hole for every mile, then the tenths are marked to 5 by knotted cord, $\frac{5}{10}$ a small red rag and then knots up to 1 mile, or the line may be divided into feet. About 60 ft. of *stray* should always be left before commencing to mark the line. Having run out the *stray*, the time is noted and the angle between the float and some fixed object whose position is determined, is taken by the sextant; after allowing the float to take out the line for the given interval, the distance run out is noted and also the angle between float and object is taken. This may be repeated at the end of every interval until the marked line is all run out. (See Fig. 3.)

In tidal waters, simultaneous observations should be taken at tide gauges in connection with the current observations for the purpose of reducing the velocities as observed either to their proper tidal hours, or to velocities corresponding to some fixed plane.

THE CAUSES OF EARTHQUAKES.*

By AUGUSTUS LE PLONGEON, M. D.

Written for Van Nostrand's Magazine.

1. WHAT IS THE CAUSE OF EARTHQUAKES? This is a question of more than ordinary importance, since it has occupied the minds of philosophers in all ages, in all countries. It is a question that has been much debated in academies and other temples of learning. And still it remains a puzzle to the learned men of our modern times.

The ancient philosophers seem to have been far ahead of us in this particular, as in many other branches of knowledge; for while they were able to predict days —nay months—in advance of the occurrence of our mother earth's convulsions, and warn their contemporaries of the impending danger, those possessed of the greatest scientific attainments in our age, are unable to recognize the premonitory symptoms and announce to the world the time when, and the place where an earthquake is to take place--notwithstanding they can read in the atmosphere all the meteorologic perturbances which occur, and prognosticate the storms and other phenomena which these changes foreshadow.

Many are the theories, quite antagonistical some of them, that have been launched on the vast ocean of speculation. All of them, no doubt, more or less plausible, resting on some scientific fact or other, have met with opposition. All have been impugnated, proved inaccurate and faulty. None has unveiled the hidden truth; and the mighty problem stands yet unsolved.

Facts are certainly not wanting to serve as milestones on the road of inquiry. And the laws that govern every phase of the phenomenon, well known to the wise men of our days, if properly applied, will cast their bright light, and illuminate the darkness that hangs over it.

Geology teaches us that, from epochs lost in the deep abyss of time, the earth has quaked; and natural philosophy, together with the discoveries of vigilant scrutinizers in the arcana of nature, have told us of the laws that govern these various manifestations of its wonderful vitality, and taught us that motion is life, and life is forever and ever.

Why, then, it may be asked, are the causes of earthquakes yet a mystery? Simply because we have entered the study of these phenomena, surrounded by the preconceived ideas, the prejudices and bias, either scientific or religious, that had been inculcated in us by the teachings of our predecessors, instead of stepping into the sacred precincts of the great temple of

*Author of "A Sketch of the Civilization of the Ancient Inhabitants of Peru and their Monuments." Author of "The Jesuits and Peru."

nature, our minds free and unshackled from all prepossessions, ready to receive the revelations of the mighty arcana with candor and good faith.

I do not pretend to be wiser than any of the learned men who have investigated the subject, for I am the last among the worshippers of science. But having studied the phenomena in the midst of the terrible convulsions that have shaken the American Continent to its very basis, of late, and applied the different facts that I have observed during many years' residence in countries subject to earthquakes, to the touchstone of the natural laws that govern their manifestation, free from all undue bias, either scientific or religious, I have tried, from my observations, to draw all possible reasonable and scientific conclusions.

It is the result of patient and careful investigations that I humbly submit to your criticism in this cursory article, with the hope that it will meet with your approbation.

I have said that :

The Ancient Philosophers were acquainted with the Cause of Earthquakes.

This might be considered a bold assertion on my part, if I had not their writings and those of the historians of antiquity to back me up.

We all know that the wanton destruction, by fire, of the 700,000 volumes of the library of the Temple of Serapis, has deprived us of the knowledge of the scientific truths discovered by the wise men of antiquity. The few works, however, that have escaped the fanatical wrath of the ignorant Mahometan chieftain, and the deplorable hastiness of the Roman general, manifestly show that the philosophers of old had indeed given their earnest attention to the study of the very question we are about to elucidate ; and that, owing to their diligent inquiries, and their knowledge of the laws that govern the phenomena, they had discovered some of the causes, if not all, of the earthquakes. We read in Philostratus, that Anaxagoras, who was thoroughly instructed in the science of the Egyptians, foretold the falling of stones from heaven, and that there should be an earthquake, *in consequence of the mud which he perceived on the surface of the wells.*[1]

Marcellinus[2] asserts the same thing, and so does Diogenes Laertius. Apollonius gave it as his opinion that the earth was composed interiorly of a mixture of bitumen and sulphur in a constant state of incandescence, and when a current of air penetrated the chinks and caverns, a fire was kindled, a flame was produced that burst out from the mountains, and streams of liquid fire; this being the cause of volcanoes and earthquakes.[3]

Jamblicus[4] tells us in his "Life of Pythagoras," that Pherecides, merely by testing of or looking at the water drawn from a well, advised the inhabitants of Samos to put themselves in safety, for they were threatened with an earthquake, which, in reality occurred.

Pausanias,[5] in his "Itinerary of Greece," whilst pretending that the earthquakes are phenomena produced by the anger of the gods, enumerates, however, the signs by which they are preceded and foreshadowed. Among these he mentions the water in the wells becoming turbid and emiting fetidity.

Pliny, the Elder,[6] in his Natural History, after speaking at length upon the subject of earthquakes, endeavors to imagine means to prevent the phenomenon, and gives it as his opinion, that to some extent it might be hindered by boring very deep wells in the countries where they are of frequent occurrence.

This same author, in another chapter[7] of the same work (and Cicero,[8] in his "de Devinatio," concords with him), says that Anaximunder foretold to the Lacedemonians, not only an earthquake, but also the falling of the summit of the Taygetus, a mountain of Lacona. The event confirmed his prediction.

In the XIII century, a monk, in order to oblige the Emperor Andronic to recall from exile the patriarch Athanasius, threatened him with divers plagues—with an earthquake among them. The earthquake really occurred in Constantinople within 3 days after the prediction ([9]).

The illustrious Buffon ([10]) speaking of

1. Philostratus—"Life of Apollonius," Book I., chap. ii.
2. Ammionus Marcellinus, Book XXII., chap. xvi.
3. Philostratus—"Life of Apollonius," Book V., chap. xvii.
4. Jamblicus—"Life of Pythagoras," Book I., chap. xxviii.
5. Pausanias—"Itinerary of Greece" (Achaic, chap. xxiv.)
6. Plinius—"Natural History," Lib. II., chap. lxxx.-lxxxi.
7. Ibid , chap. lxxix.
8. Cicero—"De Devinatio," Lib. I., chap. l.
9. Pachymer—Lib. X, chap. xxxiv.
10. Buffon—Natural History, Art. XI. On the proofs of the theory of the earth.

the proofs of the theory of the earth, relates that at Bologna, in Italy, in the year 1695, everybody saw with great surprise the waters becoming turbid 4 hours before an earthquake.

Agathino Longo (11) in an historical and physical memoir on Earthquakes asserts that an identical phenomenon took place a few days previous to the earthquake that was felt in Sicily during the month of February, 1818.

Does not Mr. Cadet de Metz (12) in his Natural History of Corsega tell us that after having observed during the month of December, 1782, very dense sulphurous vapors covering the plains of Calabria Citeriore, he came to the conclusion that an earthquake was near at hand, and predicted the catastrophe which took place at the beginning of 1783.

And lastly did not Senor Vidaure, (13) a learned Peruvian, on hearing certain subterranean noises of a peculiar character, predict 4 months in advance, the earthquake that destroyed a part of Lima, in 1818.

And I, myself, (14) predicted 6 months in advance the terrible earthquake that on the 13th of August, 1868, laid to the ground the strongly built city of Arequipa, and many others in the southern provinces of Peru.

Will you reject the testimony of so many writers and historians? Will you say with Cicero: The thing is impossible? No, I am sure—for as a scientific man you know that the arcana of nature become unfolded to our gaze more and more every day; you know also that every day some of the laws that govern its phenomena are discovered, and that nothing is impossible to the human mind in the scope of discoveries and scientific investigations.

Impossibility is the by-word of ignorance, unknown among us, the worshippers of science.

2.—THAT THE CENTRE OF THE GLOBE WE INHABIT IS NOT LIQUID FIRE

is generally admitted by most of the scientific men of our age. It does not enter within the limits of a cursory article like this to enumerate all the facts that can be adduced to prove that it is most probably a compact mass of metals and minerals, with a rather thin crust of oxides to cover it. I say thin, comparatively, of course. But to prove it I will merely speak of the heat that has been observed to exist in the different strata where man has penetrated; I will try to show that this heat is simply superficial and influenced altogether by different causes than central fires.

It is a fact demonstrated and proved that electro-magnetism is the active agent that produces all the phenomena of life that takes place at every moment of time before us, continually changing and producing new beings and new species of being. I consider that electro-magnetism is the life of this immense living body on which we exist as parasites; and that in the same manner as heat is developed in the human body through electro-magnetism's agency, which causes the blood to flow rapidly, and circulate with force, throughout the entire system, producing thereby a continuous friction, in each and every of its parts, engendering what is termed *animal heat* — this being greater where there is greater affluence of circulating fluid, and therefore a greater friction—so also are the same electro-magnetic agents the source of the internal heat of the earth. How far this internal heat reaches toward the centre, is unknown, and will most probably remain unknown forever to man.

The laws of nature are as simple as they are immutable. When studied, we find that they act alike in all things, advancing from the simple to the complex. We find nature very economical of the means employed by her to produce her creations, and most prodigal in the variety of those.

Let us take her for our guide and proceed from the simple to the complex.

The science of electro-magnetism is a comparatively new one, which will eventually lead to great discoveries, and give us the explanation of many phenomena that to the present day have remained unexplained.

The heat seems to augment progressively as we descend towards the centre of the earth. But the progression is not constant. At places the heat increases rapidly, at others very slowly. This difference has for a long time puzzled geologists. At last they have come, not to a definite, but an approximate conclusion,

11 Agathino Longo—Biblioteca Italiana, Settembre, 1818.
12 Cadet de Metz—Natural History of Corsega, pp. 138-147.
13 Vidaure—Monitour Universel, 27th of August, 1823.
14 Le Plongeon—Jesuits and Peru, p. 482.

by admitting that the heat increases 1 deg. for every 27 metres. Beudant pretending, however, that it augments 1 deg. for every 33 metres, on every point of the globe. But such is not the case, for there are, perhaps, not two places, even in the same locality, where the heat is the same at the same depth.

However, for the sake of demonstration, let us admit it is so, and that the heat increases progressively 1 deg. for every 33 metres we approach nearer to the centre of the planet. What will then be the consequence? At 3,000 metres the heat will be sufficient to cause the water to enter into ebullition. At 20,000 metres, the supposed thickness of the crust of the earth, all silicates will melt. At 80,000 or 100,000 metres, all metals, even the most refractory, will be in fusion; the diamond volatilized.

As the semi-diameter of the earth is 6,366,000 metres, the heat at the centre will then sum up the prodigious amount of 250,000 deg. of heat. Think of it—250,000 deg. Can you imagine such heat and not be yourself volatilized instantaneously.

Can any man with common sense ever believe such nonsense? Why, Beudant himself is surprised at the awfulness of the offspring of his own science, his pet frightens him, for in his "Course of Natural History" he tells us: "That if anything is capable of astonishing anyone, it is that no more catastrophes should take place in our days on the surface of our planet, particularly when we consider the enormous disproportion which exists between the diameter of the melted matter and the crust of the earth, which is only 20,000 metres. This thickness is but very small when compared to the terrestrial radius, which is more than 6,000 kilometres. On a globe of 1 metre it would be represented by 3 millimetres approximatively. That would not be the thickness of a sheet of paper on one of our ordinary terrestrial globes."

These are, verbatim, the words of the savant geologist. Yet, there is another thing that astonishes me more than that; it is that a man of his acknowledged science, if he calls himself sane, can possibly cherish and seriously advocate such an idea, now that we are perfectly conversant with the laws which govern the expansion of gases, and those which regulate the march, attractions and gravitation of celestial bodies; and Mr. Beudant knows, certainly, as well as ourselves, that only a temperature of 12,000 deg. is required to volatilize all and everything known to man on earth. If his theory was true, at 320,000 metres under our feet there would be nothing but gases floating on an immense furnace of 238,000 deg of heat to expand them more and more, and a very thin shell of 20,000 metres to contain them and resist the immense pressure caused by their increasing expansion.

Who, in the name of common sense, will admit of such an absurdity?

There is no central fire; there cannot be. What does the science of the skies tell us on the particular? Listen!

We all know that the astronomers, in order to calculate the course of the celestrial bodies, are obliged to know exactly their weight and volume. They, of course, had to determine that of the earth, in order to compute its motions in space, and its relations with its other companions and co-travellers. Their computations have been so accurate that they can determine the exact time of the apparition of comets. When eclipses, conjunctions, etc., etc.. are to take place, by the astronomical observations, taken four or five thousand years ago by the Chaldean and Egyptian priests; by those of the ancient Chinese astronomers, as that of the eclipse of the sun, mentioned in the "Chou-King," which took place during the ninth month of the year 2159 B. C., all of which within the last century has been proved perfectly correct. We have come to the knowledge that no variation whatever has taken place, in the volume of the earth at any rate, from those remote times to our days. If the planet had contracted, as some pretend, the rapidity of its rotation would have necessarily increased, and such is not the case.

The natural corollary of all these astronomical observations is that the computations of the astronomers being correct, the surmises of the geologist as to the central fires and their effects are all imaginary. But you may, and very appropriately, tell me, that, as far as men have penetrated into the earth, they have found heat increasing, and then you may ask me:

3. WHENCE THE HEAT EXPERIENCED ON THE SURFACE AND IN THE INTERIOR OF THE GLOBE?

I will answer.

I told you that I consider the earth as animated, a living being, living out its own life among its brothers and sisters of the immensity, just as any one of us among our fellow-beings. That, as we have our soul, which gives life, activity, and warmth to our bodies, so has the earth its soul, that gives it life, activity and warmth, that animates all things existing in it or on its surface. I told you that this soul is *electro-magnetism.*

I do not suppose that any scientific man will dispute me, that electro-magnetism is the agent of the attraction that celestial bodies exert upon each other; and this is the cause of their motions through space. Nor will any one deny that motion causes friction; that in its turn engenders heat and light; that electro-magnetism causes the cohesion of all the molecules whose aggregate composes the universe is a well admitted fact. It is, therefore, the *life-sustainer*, the SOUL of the whole creation, of which our reduced planet is but one of the smallest atoms.

I have said that electro-magnetism was the agent that produced the heat of the earth. Let us see if my assertion is sustained by FACTS; for only through the observation of *facts* and a clear unprejudiced mind, can we compare them together and arrive at the knowledge of their causes, and to the causes of these causes that form the catenation of the natural laws, whose understanding and interpretation is science.

1st. The earth swims in a medium—a universal fluid that fills all space. Call it by whatever name you please—ether—cosmic fluid—imponderable fluid. This something is obviously matter under a certain form. It has been asserted to be composed of 64 elements that Graham has classified into 6 series or classes. This something, being matter, offers resistance, and opposes the forward motions of the bodies that pass through it. Of course it opposes the forward motion of the earth in his movement round the sun, and through the immensity where it follows this body in its rotary motion around an unknown centre. This resistance not only causes the diurnal rotation of our planet; its conical movement, which it accomplishes in 25,868 years; its vibratory motion that produces the phenomenon known as tides; but also as the earth forces its way through the universal fluid at the stupendous rate of 30,550 metres per sec., besides the 464 metres per sec. of her diurnal rotary motion, a large amount of continuous friction is produced throughout its whole surface, but particularly at the equator, where the globe is larger. Friction, anywhere and everywhere, creates heat. It is, therefore, impossible to doubt that this is one of the causes that produces heat at the surface of the earth. That heat so generated during countless ages has progressively and steadily permeated its superficial strata, and is preserved in the inferior ones, not exposed to the external causes of refrigeration, like those above, is obvious, and men find it there when they penetrate into them.

It might illustrate my proposition by the example of the cannon-ball, that when discharged from the cannon's mouth, is cold, but after having traversed the air at the rate of 500 or 600 metres a min., is very hot at the time it reaches its destination; but the process of refrigeration begins from the surface toward the centre; and if we split it we find that the interior is yet hot while the external parts are quite cool. What is our reduced planet but a very small cannon ball, that has become heated traversing the cosmic matter, since centuries, at the frightful rate I have just mentioned?

2d. The rays of the sun are another cause of the heat of the earth. These rays are not hot; certainly not, the snows that eternally cap the highest mountains, prove it. How then can they impart heat, if they are cold? Oh! electro-magnetism again is at work there! It is true that the rays of the sun do not convey heat, but they carry light. Light puts in motion the molecules that compose the atmosphere. They rub one against the other, there is friction; friction engenders heat—the atmosphere in its lower strata being more dense, the friction is greater, consequently more heat is evolved and communicated directly to the surface of the earth. It penetrates its lower strata and there is preserved as already stated, increasing the intensity of that produced by the first cause.

It is not relevant to prove how the rays

of the sun carry light through electro-magnetic agency. The electric lamp of Servin is a good illustration of how electro-magnetism engenders light. The sun, immense reservoir of electro-magnetism, we may consider as the positive coal of Servin's lamp—the earth, another reservoir, but smaller, the negative coal; the light produced is in proportion to the distance of the two poles.

"All bodies," says Mr. Jacobi, of St. Petersburg, "are magnetic in a larger or smaller degree. The earth is a vast magnet, and so are, without a doubt, the other planets, their satellites, and the sun itself."

Then the rays of the sun, acting through or rather produced by electro-magnetism, are another cause of terrestrial heat.

3d. The internal heat of the earth is also due to the immense chemical operations constantly going on under the agency of electro-magnetism, at no great depths, insignificant even if compared to the supposed crust of the planet.

This, as far as we know, is a vast conglomeration of metallic and mineral matters, which in order to combine, only need the action of an agent. This agent is electro-magnetism. For, as says the translator of Lyell's Geology: "It would be a great error to believe that the action of electricity is powerful only when noisy and sudden. Its tacit and quiet action throughout nature is far more important. It extends its influence in nearly all combinations. *The chemical affinity itself does not seem to be but a variety of electrical attraction.* And since the constant reunion and the quasi-identity of the electrical and magnetic fluids has been demonstrated; since the phenomena of the magnetic needle, those of the thunder, of the electric fluid in the air, and its dispersion, find an explanation in the action of electro-magnetism, *well may we presume that electro-magnetic currents circulate in the interior of the globe; and indeed, experiments performed on the electro-magnetic properties of metalliferous veins, have led to the discovery of marks and vestiges of these currents in the interior of the earth.*"

We all know that chemical combinations, decompositions, and recombinations take place incessantly in the vast laboratories of the earth. These operations are nothing else but the result of the action of electro-magnetism on the molecules of matter. These are in continual motion. During their travel they evolve heat, in consequence of the perpetual friction they are subjected to. Hence the chemical operations going on in the interior of the planet are another and third cause of its heat; and the life-sustainer, electro-magnetism, the agent.

4th. The oxidation of metals is another cause of the heat of the earth.

It is a truth known and demonstrated that currents of electro-magnetism traversing metallic bodies, produce oxidation.

The earth, or at any rate that portion explored by man, is a conglomeration of mineral and metallic bodies. These are constantly traversed by electro-magnetic currents. Oxidation then takes place incessantly and produces an augmentation of temperature. Slow, it is true, but constant.

Leaving aside all imaginings, which should never be invocated in the elucidation of scientific questions, as it has unhappily too often been the case in the very one under consideration, I will try to co-ordinate the truths enunciated, and direct their light into the darkness that surrounds the mystery of the earth's convulsions, and try to discover the part they play in

4.—THE PRODUCTIONS OF EARTHQUAKES AND VOLCANOES.

What are volcanoes? Are they the safety valves which prevent our poor little planet from bursting like a bomb and sending us flying towards the skies, as some pretend?

This question I will answer by another. Who would ever imagine to inquire if the boils, that sometimes, under the influence of certain pathological conditions, appear on the human body, are the safety valves, intended by nature to prevent the explosion of that body?

If there are no central fires; if it is mathematically, scientifically, nay, materially impossible there should be any, what is the use of vents or valves? If the planet is a solid mass, what danger does it run of exploding?

But let us suppose, for an instance, with Buffon, Zimmermann, Humboldt, Cuvier, La Place, Beudant, and many other illustrious defenders of the existence of an internal ocean of fire, whose

burning waves sweep, ebb, and flow against the walls of the thin shell on which we live in imminent danger, and examine if the volcanoes can be the vents of that immense furnace.

Our first step will be to ascertain the number of volcanoes and the size of their craters.

Geographers tell us that there are 163, the positions of which are perfectly known; 67 are on continents, 96 on islands, adding this most singular fact, that none of these situated on continents are at a distance exceeding from the sea, more than 75 miles in an air line—a peculiarity that I commend to your attention.

Humboldt asserts, that they number in all 223—all active; Keith Johnston declares that there are 270—190 on islands, 80 on continents.

Never mind what these authors say about the number of volcanoes; I will grant that there are many more they knew nothing about. Let us be generous and double the number, so as not to be accused of trying to crawl through a small hole. We shall say there are 550 vents or safety valves, if, by and by, volcanoes prove to be such.

Our next step will be to ascertain the size of the chimney or crater of these vents.

Geographers again tell us that the crater of Vesuvius, one of the largest known, is 2,000 metres in circumference. Let us continue to be generous, and say that the average opening of all the craters is 3,000 metres in circumference. One thousand in diameter, the $\frac{1}{20000}$ part of the supposed thickness of the terrestrial crust. That is admitting that the chimney is of the same all the way down.

What is a hole of 1,000 ft. in diameter compared to the whole surface of the globe? It is not even as much as a hole made on one of the terrestrial spheres in use in our common school, with a fine cambric needle.

Now, by way of illustration, and in order to keep all due proportion, let us suppose one of these globes to be one metre in diameter, hollow, made of the most refractory metal—say platina—the thickness of which, in order to correspond to the thickness assigned to the supposed crust of the earth, would be 3 millimetres. We shall proceed to bore on the surface 550 small holes with a fine needle. These will represent the volcanoes or safety valves. After these we shall introduce into it, through an opening left for the purpose, to be afterward closed and the covering solidly consolidated, so as to present the same amount of resistance as the balance of the surface—two parts of filings, one and a half part of pulverized sulphur, and a sufficient quantity of salted water to make a soft paste. When the chemical decomposition will take place, and the heat so intense as to melt the whole mass into sulphuret of peroxide of iron, the water will have been converted into steam, this again into gases, do you imagine that the 550 little holes or safety valves will permit a sufficient quantity of steam or gases to escape, and prevent the apparatus from bursting? Will even any steam or gas escape through these little openings?

Any unprejudiced mind will say no. Then how can the 550 volcanoes that we have supposed to exist on the surface of the earth be considered any longer as the safety valves of the great liquid heart, said by some geologists to occupy the centre of the planet?

Their assertion cannot stand the touchstone of science. And the truth of our denial is the more obvious, if we take into consideration that not $\frac{1}{10}$ of the known volcanoes are in activity; and that those which are active do not continually throw out lava; which they would, if connected with the central furnace.

From all that I have just said I think I may safely deduce that the volcanoes are not the safety valves of the earth, but mere local accidents on its surface, just as boils are on the human body.

There is another remarkable fact connected with the existence of volcanoes. It is this. That all volcanoes, without an exception, have for a base, and are situated on, primary formations; showing that the combination of porphyric or granitic rocks with the metallic veins found in them is the only capable of giving birth to volcanoes, through the agency of the electro-magnetic currents that traverse them. It is out of the limits of this article to enter into a nomenclature of all the substances found in the superficial strata of the planet, whose nucleus seems to be formed of granite, syenite, protogine, diorite, pegmatite and porphyry, crossed in every direction by metallic veins.

It is well known that whenever two or more of these mineralogic and metallic substances are put in contact and thoroughly wetted with salt water, a chemical decomposition takes place evolving heat the—more extensive the decomposition the greater the amount of heat.

It may even reach incandescence under certain circumstances; such, for instance, as the influence of the sun or other celestial bodies, that, acting one on the other as powerful magnets, engender immense voltaic arches, scatter throughout the boundless fields of creation, light and heat.

Then wherever a large quantity of these sustances has accumulated, there will be a large amount of heat developed, which under proper conditions will give birth to a volcano. This is also one of the causes of the multitudinous variations that are observed in the temperature of the divers parts of the earth, even at the same depth.

It is a fact very evident, that if that heat had its source in a central fire that would emit an inconceivable quantity of caloric, the thin crust would be equally heated, even to the top of the highest mountains; seeing that the Guarisconkar, in the Himalaya, said to be with the Illimani and Sorata in Bolivia, the highest peaks of the globe, is only 9,000 metres in height, that is to say, the fourteen-hundredth part of the earth's diameter.

5.—ARE THERE ANY MEANS OF DETECTING THE PLACES WHERE SUCH ACCUMULATIONS EXIST?

Seems to be the next very natural query that presents itself to the mind.

If you read carefully the history of all the volcanic eruptions; of all the great earthquakes that have laid to the ground the habitations of man, and buried the inhabitants under their ruins, you will find that premonitions have passed unheeded, because there were no inquiring, no scrutinizing minds to take note of them, and draw from them the proper inferences. In many instances sulphurous vapors have been seen arising from the ground; strange and mysterious underground noises are heard inspiring awe and terror alike in men and beasts; mineral waters are seen to be altered; soft waters are seen to become turbid in wells; the level of these waters is changed. Sometimes even the wells become perfectly dry, without any apparent reason. In caves, cellars, excavations, carbonic acid gas is noticed to emanate from the soil; magnets lose their power according to the force of the impending convulsion.

Experience has taught men to recognize in these phenomena, the symptoms of a mighty underground work—the signs of some terrible cataclysm near at hand—and are the same observed in the artificial volcano of Emery. Pliny, the Junior, in a letter to Tacitus describing the death of his uncle, ascribed it to suffocation, caused by the sulphurous vapors emanating from the ground, and calls them the forerunners of the catastrophe. It is well known that the sulphur mines are invariably found in close proximity to volcanoes—nay, in their very crater.

I have said that on the Continents none of the volcanoes are at a distance from the sea exceeding 75 miles in an air line. By some means or other the waters of the sea penetrate into the interior of the earth, since the vapors arising from the lava and the smoke coming out of the craters are the same as would result from the decomposition of salt water, and deposes large quantities of chloride of sodium. It is then that the salt water, coming in contact with the primary materials, causes the chemical decompositions and the generation of sulphurated hydrogen gas.

The sulphurets of potassium or sodium possess the property of decomposing the water at the mean temperature.

I will try to explain.

(To be continued.)

THE report of the surveyor employed to run the line of the proposed Canadian-Pacific Railroad has recently been published. The distances, as stated by him, are as follows: Montreal to Ottawa, 115 miles; Ottawa to Mattawan, 195 miles; Mattawan to Fort Garry, 985 miles; Fort Garry to Yellow Head Pass, 985 miles; thence to the limits of British Columbia, 52 miles, route by the Upper Fraser (British Columbia), by "short cut," 445 miles. Total length from Montreal to the Pacific, 2,777 miles. These distances are in large part estimated, as there has been as yet no survey of the whole route.

THE THEORY OF THE HOT BLAST.

By I. LOWTHIAN BELL.

From "Journal of the Iron and Steel Institute."

In the year 1828, J. B. Neilson patented an "improved application of air to produce heat in fires, forges, and furnaces, where bellows or other blowing apparatus are required." This discovery consisted, as is well known, in heating the air before it is propelled into the furnace; and although, from the title of the patent, Neilson and his colleagues appear to have expected to see it generally employed in all furnaces driven by compressed air, its use has, practically, been exclusively confined to those employed in smelting the ores of iron.

In 1834, Monsieur Dufrenoy was sent over to this country, by the Director-General of Mines of France, to report to the authorities at Paris on an invention, which at the time was truly described as one revolutionizing, in Scotland at all events, where it was first put into practice, the art of making iron.

This gentleman in a report gave good reasons apparently for this statement, by quoting the experience of the owners of Clyde Iron Works, which was as follows:—

For the year.....	1829	1831	1833
Temperature of blast....	Cold.	450° F.	612° F.
Coal used per ton of iron.	As coke.	As coke.	In raw state.
For fusion, cwt.... ..	133	86	40
" heating air, raw coal.	nil.	5	8
" blowing engines "	20	7	11
	153	98	59
Cwt. limestone per ton of iron.....	10½	9	7

From this it would appear that heating the air with 5 cwt. of coal had saved 47 cwt. of fuel in the furnace, and 8 cwt. similarly applied had been followed with an economy of 93 cwt., or above 69 per cent.

Besides this advantage, the make was increased by more than one-third, and a blowing engine, which only supplied three furnaces with cold blast, was equal to four when the air was heated.

The iron trade hesitated somewhat in crediting that the heat generated from 8 cwt. of fuel burnt outside the furnace, should be able to perform the duty of a very much larger weight burnt inside. Some writers on the metallurgy of iron, when speaking of the advantages of Neilson's system, have perhaps not been sufficiently careful in drawing a distinction between the saving directly due to its application and that arising in a collateral manner from its use. Looking at the question, however, in its commercial sense, the figures and language quoted from the work of Dufrenoy justified the character he gave to it.

There is undoubtedly, as this writer alleged, a saving, and, in the case of the Scotch furnaces, a very great saving of fuel by the use of the hot blast, exceeding considerably that of the weight of coal expended in the hot-air apparatus; but it seems a mere waste of time to endeavor to assign a cause, *in a heat-producing point of view*, why with the blast at 450 deg. F., obtained by burning 5 cwt. of coal, 93 cwt. of fuel should do the work of 153 cwt. with cold air, for the simple reason that it is incorrect so to state the economy effected by the invention. Thus, the burning of 7 cwt. instead of 20 cwt. of coal, per ton of iron, under the blast engine boilers, does not affect beneficially the quantity, nor the application, of the heat developed in the furnace itself.

Again, according to Dufrenoy, the coal used at the Clyde Works contains 64.4 per cent. of coke; but in the statements of the consumption he gives, there was used, per ton of metal, at the furnace blown with—

		Raw coal.
Cold blast	60 cwts. of coke,	obtained from 133 cwt
Hot blast, 450°F.	38 " "	" " 96 "
Difference....	22 " "	Difference.. 37 "

These quantities of coke, viz., 60 and 38 cwt., in reality only represent 93 and 58 cwt. respectively of raw coal, the difference between these numbers and those quoted above being waste of fixed carbon in the coking process. Hence, although it may be perfectly correct, commercially speaking, to say there is a gain of 37 cwt. of coal, in a heat-producing point of view, it is only 22 cwt. of coke we have to set against 5 cwt. of coal burnt

in the hot-air stoves. This margin of 17 cwt. (the difference between 22 and 5 cwt.), however, is sufficiently remarkable, and various explanations have been given to account for the apparent anomaly. Some of these I propose to examine in the present section, and then to consider the question with the assistance of the experiments and reasoning made use of in my own investigations on the action of the blast furnace.

The late Dr. W. Allen Miller* conceived the economy effected by the use of the hot blast to be due to the reduction of the ore taking place nearer the crucible, and thus concentrating the heat.

The analyses of the gases at different depths of the furnace, quoted in these pages, when blown with cold air, and with that varying from 180 deg. C., to near 500 deg. C., do not appear to afford any countenance to the opinion advanced by this chemist.

Dr. Clark, Professor of Chemistry in Aberdeen, examined, in 1834, the action of the hot blast, and assigned as the cause of the saving of fuel that an ordinary furnace received 6 tons of cold air per hour, which he regarded as a tremendous refrigeratory passing through its hottest part, and thus repressing the temperature required for the complete and rapid reduction of the iron.

This explanation of the cooling effect of the air is true enough, but it scarcely accounts for the fact that one unit of heat in the blast was at that time saving something like four derived from the fuel burnt in the furnace. Dr. Clark, too, appears to have entertained the same opinion as that expressed some thirty years afterwards by Dr. Miller, viz., that the reduction of the iron was an operation performed in the hottest part of the furnace, whereas it would seem, from the analyses given in a previous part of this work, it is one almost exclusively effected in the coolest region.

Mr. Truran, in his work,† maintained that all writers previous to his time had greatly exaggerated the effects of the hot blast in the manufacture of iron. Dr. Percy‡ has effectually disposed of the chemical reasoning upon which this author supported his assertion. I am not aware that any one pretended that its introduction into Wales had been attended with the same beneficial results which distinguished its use in Scotland, and Mr. Truran certainly did not succeed in proving that Neilson had not, by his invention, afforded very valuable assistance in smelting the black band of that country.

Sir Wm. Fairbairn, in his work on the manufacture of iron, suggests the propriety of investigating the alleged consumption of fuel in the throat of the furnace, to which Mr. Truran attributed certain effects of the hot blast. Sir William himself seems to be under the impression that narrowing the throat increases the effect of the blast in this region. As no portion of the blast ever reaches beyond a few inches from the tuyeres in the form of atmospheric air, whether it is employed hot or cold, and whatever be the shape of the furnace, this view of a change in the nature of the combustion seems also devoid of any foundation.

Dr. Percy, who has examined with great care and minuteness the writings of almost every author, English and foreign, on this question, states* that, "after the positive, oft-repeated, and generally credited statements which have been put forth concerning the extraordinary effect of the hot blast in diminishing the consumption of fuel in the smelting of iron, it might seem superfluous to raise any question as to the fact."

The extraordinary effect alluded to by this author would appear to have more special reference to such savings as are contained in the published accounts of the late David Mushet, who gives 148 cwt., of coal coked for making a ton of iron with cold air, against 43½ cwt., used raw, with hot air, at the Clyde Works.

Dr. Percy considers that the abstraction of heat by the expansion of so much cold air when it enters the tuyeres must, by the mere act of dilatation, produce an unfavorable effect on the condition of the furnace, and that heated oxygen, combining more rapidly with incandescent carbon, willl give a temperature of greater intensity in the direct ratio of rapidity of combustion. This being so, he adopts, for the sake of argument, the supposition that a metal requir-

* "Elements of Chemistry."
† "Iron Manufactures of Great Britain," 1855.
‡ "Metallurgy of Iron and Steel."

* "Metallurgy of Iron and Steel," p. 418.

ing 1,000 deg. C. for its fusion, might be subjected to a temperature of 999 deg. C. for ever without melting. So it may be, the Doctor continues, in the blast furnace with respect to the carburization of the reduced iron, and certain other accompanying chemical actions, which may take place with slowness at one temperature, and with rapidity at another, slightly elevated. In order to produce these actions in a furnace on cold blast, it is requisite to consume a much larger quantity of coal than in a furnace on hot blast. A few degrees of temperature may make all the difference. As a further proof that it is *calorific intensity* which constitutes the superiority of the hot over the cold blast furnace, Dr. Percy mentions that in both cases the fuel is wholly consumed, and as the gas also which escapes from the furnace mouth has substantially the same composition, it follows that the *amount* of heat generated in a furnace working with cold blast is enormously greater for a given weight of pig iron than in one working with hot blast, the conditions with respect to quality of ore and fuel, dimensions of the furnace, etc., being supposed to be the same in both cases.

A word or two with regard to the absorption of heat by expansion as the blast enters the furnace, and the identity of the composition of gases from hot and cold blast furnaces.

Admitting that a current of cold air absorbs more heat by its expansion than one of hot air, both having the same pressure, will this absorption, whatever it may be, not be met by the addition of prec sely the same quantity of heat if communicated to the blast itself? If so, we are not called upon to explain, so far as this item is concerned, any discrepancy between the heat communicated to the air, and the actual effect it produces in the furnace; for there would be no difference between the two. As regards the composition of the gases, it is difficult to determine, from mere reasoning, what this would be. The action of the blast on the fuel would undoubtedly, in both cases, give CO; and the actual quantity of CO_2 produced by reduction and carbon impregnation would be the same in the hot as in the cold blast furnaces, but what proportion of this CO_2 might suffer reduction to the state of CO is not so clear; but supposing it also were the same in both furnaces, inasmuch as the cold blast furnace is burning much more C to the condition of CO, and cannot convert more of this CO to CO_2 than happens when heated air is used, it follows that it is highly improbable that Dr. Percy's prediction as to their identity of composition would be realized.

Dufrenoy gives it as his opinion that the virtue of the hot blast is to be ascribed to the higher temperature of the furnace, and in support of this hypothesis, he adduces the fact that less limestone is used than when working with cold air, and hence "this diminution of the fluxing matter is the strongest proof that can be given of the temperature of the furnace. It proves that the earthy particles undergo a degree of heat powerful enough to fuse them, with the addition of a smaller quantity of flux."

This view of the condition of the zone of fusion is also that entertained by Dr. Percy, who reminds us that in looking into the tuyere of a cold blast furnace, the interior is black, instead of presenting the dazzling bright appearance of one blown with heated air.

As regards the diminution of limestone, there are other objects to be gained than the mere fluxing of the earthy constituents of the ironstone, such as the removal of sulphur, etc. This would appear to be so, because it often happens that the earths, in the proportions in which they exist naturally in an ore, constitute a readily fusible slag without the addition of any flux; thus, according to Colquhoun, in Scotch Black Band, we have them as follows:—

$Si O_2$ 38, Al_2O_3 21, Mg O 18, Ca O 23=100.

Exp. 743. A mixture of these four substances in the specified quantities was exposed to the heat of a smith's forge, when it melted readily into a vitreous slag; and similarly, when they were in the proportions in which they are found associated in the Cleveland ironstone, they afforded a beautiful white glass.

The supposed duty of lime in removing sulphur, and it may be phosphorus to some slight extent, often leads to its use where the quantity, instead of adding to the fusibility of the slag, has a precisely opposite effect. Such, at least, is apparently the case in smelting the ironstone of Cleveland, judging from the slag formed upon one occasion, when, by way of ex-

periment, all limestone was withdrawn from the charges. The slag was well fused, but the iron became hard, and contained above three times the usual content of sulphur.

The darkened appearance of the hearth, spoken of by Dr. Percy,* is susceptible of another explanation than that of an actual general refrigeration of this region. It is more probable, it occurs to me, that the cold air, meeting the slag and iron, chills a small portion of the fused matter as it passes the tuyeres; but this, of course, is not the action of air on carbon, but of air upon slag and metal. When, on the other hand, the melted contents of the furnace trickle down before a current of heated air, they also, no doubt, are chilled, but not to the extent to deprive them of fluidity before they pass beyond its influence; and, in this way, there is no accumulation of solidified matter, which gives rise to the prolonged tube or "nose," as it is termed, reaching far into the hearth.

To account for this supposed increase of intensity in the heat of a hot-blast furnace, Dr. Percy quotes the experiment of the Swedish metallurgist, Mr. Sandberg, who observed that heated oxygen combines more rapidly with incandescent carbon than cold oxygen, and that in consequence there "must be a proportionate increase of temperature, for, *cæteris paribus*, temperature will be in the direct ratio of rapidity of combustion."

Ordinary combustion of carbon is generally spoken of as an oxidizing of the carbon about to be burnt, and this no doubt, looking at the office played by oxygen in combustion generally, is a convenient mode of expressing the action. But as combustion of carbon is no more the oxidation of this element than it is the carburizing of oxygen, it will be more convenient, for our purpose, to speak of it in this inverted manner. Now, when oxygen is propelled into *a blast furnace*, it becomes rapidly carburized to its utmost limit of saturation, *i. e.*, it is speedily converted into CO. This union with carbon is so instantaneous that free O is never mentioned as the constituent of any analyses of furnace gases I have met with or made. Hence if there was any difference in the extent to which the oxygen became carburized as between cold and hot blast, it would manifest itself in the larger quantity of CO_2, where the rapidity of combustion was the least, because in this case the oxygen is unable to take up as much C as when the union is more speedy, and this slowness of combustion is what Dr. Percy concludes will happen when the oxygen is driven into the furnace as cold blast.

Were it consistent with observation that this carburization of oxygen were retarded by the want of heat in the oxygen, I would submit that the very opposite effect to that mentioned by this writer would ensue at the tuyeres, *i. e.*, instead of a cold blast furnace having a lower temperature generated in the hearth, it would be much hotter, inasmuch as the heat evolved by carburizing O to the lower state of CO_2 is three times that produced by the more perfect carburizing of the blast, which happens when CO is the product.

We need only refer to what has been said in Section XXX. on the origin of the heat in the blast furnace, to be satisfied that no temperature to which it would be possible to raise the air could compensate for such a difference as exists between carburizing O to the condition of CO_2 and CO, 8,000 calories being the product of one unit of carbon passing to CO_2 against 2,400 when the other oxide is produced.

The following analyses, given in Dr. Percy's work, show the volumes of CO and CO_2 per 100 volumes of the gases:—

Furnace.	Blast.	Gases collected at	CO.	CO_2.
Clerval—cold	.....	Tymp...	39.86	.93
Do.	190°C. (374° F.)	18 in. above tuyeres	41.59	.31
Eisenerz,	200° C. (392° F.)	Level of tuyeres	22.06	11 60

Is there, however, any valid ground for asserting that the carburizing of the injected oxygen is slower when it is cold than when hot? In the analysis, referred to by Dr. Percy himself, of the gas taken from the tymp (about the level of the tuyeres) of the Clerval furnace blown with cold air, there are 39.86 vols. CO to .93 vols. CO_2. At the same place, when the blast was used at 190 deg. C. (374 deg. F.), a specimen of gas 18 in. above the tuyere, and therefore vertically about as far from the point of entrance of the O as the other was horizontally, 41.59 vols. of CO were accompanied by .31 vols. of CO_2. This difference, small as it is, must be regard-

* "Metallurgy of Iron and Steel," p. 427.

ed rather as accidental than general, and under no circumstances can account for any great change in the actual temperature.

The information, however, elicited by an analysis of the Wear gases, taken from the level of the tuyeres, shows that carburization of the oxygen is not more rapid with the blast at 460 deg. C. (860 deg. F.) than it was at Clerval with cold air.

		CO		CO_2
Experiment	744	37.5	.....	.76
"	745	37.7	.....	76
"	746	35 8	.....	2.10
"	747	31.7	.....	1.1
"	748	33.8	.....	.8

All this argument, nevertheless, would have to give way to the fact, could it be established, that the temperature of the hearth of a hot-blast furnace really did exceed that of one blown with cold air.

I have already (Section XXXVI.) alluded to the difficulty experienced in obtaining, by means of the calorimeter, any data sufficiently uniform to enable me to determine the temperature of the contents of the hearth of a furnace. In the same place, however, I have given my reasons for believing that the cause of the difference between white iron and grey was simply one of temperature. This was done, it may be recollected, by heating white iron to a point known to be sufficient to produce grey iron, and the change was effected, *i.e.*, the white became converted to grey. The plan followed consisted in plunging a bar of white iron in a current of slag, as it issued from a furnace producing iron grey in quality.

Since writing the account of this experiment I have, by the kind assistance of my friends, the Messrs. Kitson, of the Monkbridge Works, at Leeds, satisfied myself of its correctness by another mode of procedure.

Exp. 749. About 60 lbs. of Clarence white pig iron was melted in one of their Siemens steel melting furnaces, and run into a mould of green sand forming a cube of 6 in. on a side. With the exception of a trace of white at one corner, the whole was converted into uniform good grey forge.

Exp. 750. The same quantity of the same iron as that used in the previous experiment was melted on a Saturday, and allowed to remain over the Sunday in the furnace, during which time it cooled slowly. The block, having a maximum diameter of 6 or 7 in., was entirely grey, chiefly No. 3, interspersed with some No. 4.

It seems, therefore, not unreasonable to accept the iron made in any particular furnace as a species of pyrometer for determining, if not the actual temperature of its interior, at all events of establishing a comparative test between it and that of another.

If, then, temperature is the governing cause of quality of iron (numbers), it is highly improbable that the same degree of heat intensity is, *cæteris paribus*, making white iron in one furnace and grey iron in another. It does seem more rational to suppose that if two furnaces are both running, say No. 3 iron from the same materials, one blown with hot air and the other with cold, they are making the same quality, because the temperature is the same in both.

This admission as to parity of temperature, of course, does not presuppose that the heat evolved by the combustion of carbon with hot air is not more intense than that arising from cold air; but what I affirm is, that, in consequence of the greater amount of iron and slag being melted with a given weight of fuel, when burnt with hot blast, the actual temperature in the crucible of such a furnace, is probably lowered to that of one blown with cold air having a smaller weight of material to use.

In recent years, by raising the temperature of the blast to 485 deg.C., the consumption of coke has been reduced to 28 cwt. per ton of iron when smelting Cleveland ironstone in a furnace 48 ft. high, and in which probably more than 60 cwt. (the quantity named in connection with the Scotch furnaces) would at least be required with cold air. Of the carbon in the 28 cwt. of coke mentioned above, after deducting that dissolved by the CO_2 of the flux, about 25 cwt. would be burnt to CO at the tuyeres.

25 cwts. C. × 2,400	= 60,000 calories.
Blast will contain about.	16,000 "
	76,000 "

The intensity, however, of this quantity of heat has been shown in Section XXXVII. to be greatly augmented by that intercepted by the materials and brought down again to the zone of fusion.

This was in one case proved to amount

to 70 per cent. of that actually evolved by the quantity of fuel consumed to produce one ton of iron. So far as intensity therefore is concerned, we have to deal with

Units from coke per ton of Iron.....	76,000	calories.
Plus 70 per cent. intercepted and brought back.....	53,200	"
	129,200	"

In like manner, with a consumption of 60 cwt. of coke, about 54 cwt. of carbon would be burnt at the tuyeres to CO.

54 cwt. C × 2,400 .... =	129,600	calories.
Plus heat in blast about	2,400	"
	132,000	
To which if we add for intercepted heat 70 per cent.....	92,400	"
There is obtained.....	223,400	"

Now, it seems, on the face of it, incredible that the mere addition of 16,000 cwt. heat units, in the place of 2,400, should so alter the intensity of heat in the crucible as to make all the difference in the work performed.

Upon a former occasion* I called attention to the fact that whereas in the hearth of a blast furnace the C. was burnt to CO, in the hot-air stoves it left the fire-place as CO_2. This of itself would enable each unit of carbon to give 3.33 times as much heat when burnt in the hot air apparatus as when burnt in a blast furnace. The advantage, however, resulting from this change in the manner of combustion, would be considerably diminished by the great loss from radiation, and at the chimney of the heating stove. This is conclusively exhibited by the fact that whereas at least 4 cwt. of coal, equal to 32,000 cwt. heat units, are used per ton of iron in the stoves of a 48-feet furnace, only 16,000 find their way through the tuyeres.

Upon the same occasion I alluded to the circumstance that the mere alteration of the proportion of ironstone to coke conferred an advantage upon the hot-blast furnace, by presenting to the ascending gases a greater quantity of matter having a higher heat-absorbing power, ironstone being superior in this respect to coke.

Exp. 751. To determine the relative powers of coke, limestone, and ironstone, a cast-iron cylinder was provided, 4 ft. long and one ft. in diameter, closed at each end. The material to be operated on, broken as nearly as possible to the same size, was introduced into the cylinder, which it filled, and by means of an inch pipe hot air was introduced at the low end and allowed to escape at the upper. This was continued until a thermometer inserted at the top became stationary.

Numbers of a very constant character were obtained when using the same substance, and from these were deduced that bulk for bulk, taking coke as unity, the intercepting power of

Coke being.....	1.00
Cleveland calcined ironstone was.....	2.00
Limestone was.....	1.60

These figures, however, indicate that the heat-absorbing power of a hot-blast burden is only about 10 per cent. superior to that of cold blast.

Under these circumstances, therefore, it is clear that neither modification of the circumstances just mentioned can account for more than a very small proportion of the actual saving effected by the use of heated air.

In the treatise by M. Dufrenoy, pointed allusion is made to a fact in connection with the use of hot air in France when using certain ores, from which it would appear that practically no saving whatever resulted from its application to the smelting of the mineral used at La Guerche* which, although containing only 42 per cent. of iron, gave a ton with 25 cwt. of fuel and cold air.

Mons. Dufrenoy dismisses, what seems to me a very important matter in connection with the theory of the hot blast, with the simple observation that the figures, in connection with the Guerche furnace he quotes, are "not favorable to the use of hot air," and I am not aware of their having attracted the notice of any subsequent writer on this interesting question.

It may be remarked that the "extraordinary saving" mentioned by most authorities as consequent upon the use of the hot blast seems to have been regarded in the light of one of general application.

* "Chemistry of the Blast Furnace. Transactions Chem. Soc. of London, 1869."

* This furnace was compared, by Dufrenoy, with a neighboring one at Torteron, using the same kind of fuel and ore, to which heated air was applied. The only change caused by the alt red mode of working was, that the iron, instead of being white, as it was when cold air was used, became grey.

At all events, little stress has been paid to the well-known fact, that in many instances the economy of fuel was considerably less than in Scotland, where its much greater importance has, so far as is known, formed the usual ground upon which its powers were considered.

Dufrenoy himself, for example, mentions the circumstance that at the Plymouth Iron Works, near Merthyr Tydvil, a ton of iron was produced with 53 cwt. of raw coal, and that by the use of hot air it could be reduced to 36 cwt. The saving, 17 cwt. of Plymouth coal, only represents 13 cwt. of coke, instead of the 22 given as that effected in Scotland by hot air.

Now, the problem I would suggest to those who allege that "calorific intensity in what may be considered the most active part of the furnace is higher in the case of hot blast than in the case of cold blast" is, whence arises it that the addition to this assumed intensity of heat in the crucible is attended with such different results as those just mentioned?

I cannot help thinking that the answer to the above inquiry is to be sought for in an entirely different direction, and, in pursuit of which, may be quoted the results of certain experiments described in the earlier sections of this work. In these it was conclusively proved that differently prepared specimens of oxide of iron and different kinds of ore, all being peroxides, were very differently affected by the application of heated CO. Thus, in about 7 hours, at a temperature of 410 deg. C. (770 deg. F.), in

Exp. 30.	Fe_2O_3 from calcined nitrate, lost.	72.7	Per cent. of its original O.
" 29.	" precipitated.....	66.7	
" 23.	" from calcined $FeSO_4$	61.7	
" 35.	Lancashire hematite.....	57.4	
" 34.	Calcined spathose ore	28.4	
" 33.	" Cleveland ore	20.7	
" 26.	" " "	9.4	

Now, the two experiments 33 and 34 may be taken almost as an exact type of what happens in the Plymouth and Scotch furnaces respectively. We have calcined spathose ore losing its oxygen with nearly one-half more rapidity than that of the Cleveland hills. This would mean that for the purpose of complete reduction we should have to expose the last-mentioned mineral for a one-half longer period of time than the other.

Let us see how this difference in susceptibility to reduction accounts for the greater consumption of fuel in the case of the black band, as compared with the ore smelted at the Plymouth Works, both of which will be regarded as yielding the same percentage of iron.

In the first place, it is obvious that it cannot be from any difference in the cooling effect of expanding air, as the blast is cold in both cases; neither can it be rapidity of combustion causing intensity of heat, because, if both furnaces burnt the same quantity of fuel in a given time, the Scotch ore would still continue to require the larger quantity of combustible per ton of iron.

It can hardly be alleged that the same quantity of iron and slag to be melted, derived from black band, can require for their fusion in the hearth one-half more heat than do the same substances from the mineral operated upon in South Wales.

Instead of all this, let us suppose that in the hearth of a blast furnace a certain mixture of reduced iron and earthy matter had to be fused, with the minimum quantity of fuel capable of evolving, with cold air, the necessary heat. From the combustion is generated a certain quantity of CO, which we will further suppose to be able to carry off all the oxygen contained in the minerals, without difficulty. This carbonic oxide now commences to flow through the oxide of iron at a rate determined by the rapidity of the combustion and fusion at the tuyeres.

Reduction is effected by the current of CO, and in the case of the Plymouth mineral it takes place at such a rate that by the time about 40 cwt. of coked fuel, the produce of 53 cwt. of Welsh coal, are burnt at the tuyeres, deoxidation has been completed, and carbon-impregnated iron is ready for fusion.

If, however, another ore, say black band, parts with its oxygen much more slowly than that just described, it is certain that the exposure to the reducing agent must, by so much, be prolonged. This, however, cannot be accomplished if the rate of fusion continues the same as was in the Welsh furnace. There is, therefore, no alternative, in a small furnace, but to retard fusion by the addition of fuel. This reduces the weight of material to be melted, and at the same time supplies an *extra* quantity of CO, which also assists in overcoming the want of

susceptibility of reduction inherent in the more refractory ore.

The law, therefore, which I believe determines the whole question of differences of fuel required to smelt ores of different kinds, but containing the same percentage of metal, and which constitutes the value of the hot blast, is—*that the rate of reduction must not proceed less rapidly than that of fusion.*

It must not be imagined, if a sample of the Scotch black band and one from Plymouth were exposed to a current of heated CO, that deoxidation would necessarily take place exactly at the rates indicated by the quantity of coke required respectively to smelt them; at the same time a fair idea would in all probability be obtained by the information afforded by such a trial.

In the hearth of the Scotch furnace there will be evolved for each ton of iron something like 132,000 cwt. heat units, against 88,000 at Plymouth, both being blown with cold air. It will be observed that in both cases the actual quantity of heat is greatly in excess of what can be possibly required for fusion of iron and slag. This excess, after satisfying the requirements of the crucible, is carried off in the gases, a much larger quantity, of course, in that where the consumption of fuel per ton of iron is the largest. The application of the law just laid down to the two examples now considered, consists in supposing that under the circumstances of size of furnace, etc., whereas 60 cwt. of coke was needed in Scotland to bring the reducing and fusing powers in harmony with each other, the coke from 53 cwt. of raw coal (about 40 cwt.) sufficed to effect the same object at Plymouth.

It may not be altogether out of place, before proceeding further with the present argument, to examine a little in detail the performance of the Guerche furnace, which, although of very small dimensions, when using ore of about the same richness as that of Cleveland, and requiring the same quantity of limestone, produced with cold air, a ton of metal for 24.92 cwt. of fuel.

This of course means that, notwithstanding the small capacity, the ore employed surrendered its oxygen so freely that the current of gases proceeding from the above-named quantity of fuel, became saturated with oxygen by the time they reached the throat, and quite as rapidly as the carbon burnt at the tuyeres could fuse the reduced metal and slag.

No account is handed down to us of the composition of these gases; indeed, at the period of M. Dufrenoy's observations, this subject had received but small attention. All we can, therefore, do is to consider how far the quantity of fuel consumed can be made to correspond with the actual amount of heat required.

The fuel employed consisted of two-thirds charcoal and one-third coke, but as the work done at Guerche was compared with that of another furnace (Torteron), also using the same mixture of combustible, but blown with hot air, and as we are now considering the quantity of heat, which is the same practically from coke and charcoal, the presence of the latter may be disregarded.

Fuel consumed per ton of iron	24.92 cwt.
Ore	48.74
Limestone	11.16

Estimate of heat development—

Fuel burnt	24.92	
Deduct, say 10 per cent. for impurity	2.49	
	22.43	
C in limestone carrying off equal weight from fuel ($CO_2 + C = 2\,CO$)	1.32	
		Units.
C burnt to CO	$21.11 \times 2{,}400$	= 50,664
C of this CO burnt to CO_2, say.	$6.00 \times 5{,}600$	= 33,600
Heat units in blast from compression, say		2,000
		96,264

Heat absorption as per table in Section XXVIII.

Evaporation of H_2O in fuel	312
Reduction of Fe and carbon impregnation	34,548
Expulsion of CO_2 from limestone 11.16×370	4,129
Decomposition of this CO_2 $1.32 \times 3{,}200$	4,224
Do. H_2O in blast	2,720
P, S, and Si reduced, assumed one half*	2,087
Fusion of iron and slag	23,100
Transmission of heat through sides	3,658
Tuyere water	nil.
	74,778
Leaving (which is more than ample for escaping gases)†	11,486
	96,264

Thus it will be observed that even with cold air, under favorable conditions, a ton of iron can be obtained from an ore of only medium richness with 25 cwt. of fuel.

* Probably less P and Si than in Cleveland iron.

† In all probability, 6 of C, burnt to CO_2, is 1 cwt. in excess of actual quantity, which would reduce heat in gases to 5,886 units.

(To be continued.)

REPORTS OF ENGINEERS' SOCIETIES.

INSTITUTION OF CIVIL ENGINEERS (LONDON).—March 5, Thomas Hawksley, Esq., President, in the chair. The paper read was "On the Kind-Chaudron System of Sinking Shafts through Water-Bearing Strata, without the use of Pumping Machinery," by Mr. Emerson Bainbridge, Assoc. Inst. C. E.

Of the total expenditure necessary to open out a coal-field, one of the chief items of cost was caused by the heavy expenses incurred in sinking the shafts, and when such sinking happened to pass through water-bearing strata, the proportion due to this head, of the total cost, was much increased. When a shaft exceeded 200 or 300 yards in depth, and when the water occurred near the surface, it was usual to keep the water back by the insertion of cylindrical metal "tubbing," placed upon a hard bed of rock at a point immediately below the lowest feeder. Where pits were less than 100 or 200 yards in depth the application of tubbing was not of much service, as the movement and dislocation of the strata, consequent upon the removal of the coal, generally caused the water to find its way into the underground workings. The sinkings in which there was the largest quantity of water had been carried in Belgium through the chalk, and in England through the Permian series; these rocks usually being sufficiently porous to contain large volumes of water. Without exception, in England all such sinkings had been made by the use of pumping machinery of sufficient power to keep the pit, during the process of sinking, comparatively dry.

It was stated that the question of dealing with wet sinkings in the most economical manner would, before long, become of much greater importance than heretofore. In the report of the Royal Coal Commission an estimate was given of the coal remaining in the British Islands, as follows:—Coals yet remaining which is or will have to be reached by sinkings through the coal measures, 90,527 million tons; coal yet remaining which is or will have to be got by sinking through the Permian and other formations overlying the coal measures, 104,418 million tons; total, 194,945 million tons.

It thus appeared that 104,418 millions of tons, or 54 per cent. of the remaining resources of the British coal-fields, would have to be reached by pits sunk through the Permian and other formations more recent thon the coal measures; and, as a rule, more likely to be saturated with large volumes of water. With such important evidence bearing on the future of coal-mining, it had been considered that the present was an opportune moment to bring under the notice of the Institution a description of a mode of sinking shafts through water-bearing rocks which had proved successful in many cases on the Continent.

The plan of sinking pits hitherto practised in this country consisted in dealing with the water by means of large pumping engines, in leaving the bottoms of the pits dry enough to allow the sinkers to block the well, and in keeping back the water in the upper strata by metal rings, cast in segments about 4 ft. long, and connected by wooden joints, which were wedged tight, when all the tubbing was fixed. The evils of this system were:—(1) The heavy first cost of the plant, when special pumping machinery was used. (2) The expense of the wedging tubs, and the cost of fixing them. (3) They delay caused by the sinkers being compelled to work always in water. (4) The high first cost of the tubbing and of fixing it in the shaft, and the liability of the tubbing leaking in consequence of the numerous joints.

In the application of the Kind-Chaudron system these evils were to a great extent avoided. The system consisted of a combination of Mr. Kind's well known apparatus for boring wells, with an ingenious device, invented by M. Chaudron, for fixing cylindrical tubbing under water in such a manner as to make it quite secure and water-tight. In the latter part of 1871, the author, accompanied by Mr. W. Cochrane, visited the Maurage pits, near Mons, where two shafts were being sunk by this process. These shafts, though having a depth respectively of 373 ft. and 593 ft. at the date of that visit, had been bored that depth under water with a diameter of 13 ft. 6 in., the water having been constantly standing at a depth of 37 ft. from the surface. The Chaudron system consisted of the following distinct processes:—(1) The erection of the machinery on the surface. (2) The boring of the pits to the lowest part of the water-bearing strata. (3) The placing of the tubbing. (4) The introduction of cement behind the tubbing to complete its solidity. (5) The extraction of the water from the pits, and the erection of wedged cribs to secure the moss-box. The machinery on the surface consisted of a capstan engine, which raised the debris from the pits, and a vertical engine, by means of which the boring tools were lifted at each stroke, the speed of the latter engine varying from 15 to 18 strokes per minute. The first tool applied was the small trepan, which weighed 8 tons and bored a hole 4 ft. 8¾ in. in diameter, the depth of the boring being increased at the rate of from 6 ft. to 10 ft. per day. The pit was enlarged by a trepan weighing 16½ tons, which increased the size to 13 ft. 6 in., and was kept from 10 to 30 yards behind the pit made by the smaller trepan. The larger boring tool had 28 teeth and the smaller tool 14 teeth, each tooth weighing 72 lbs. The boring by the larger trepan did not progress faster than about 3 ft. per day of 24 hours. The boring was generally carried on during the day, the remaining 12 hours being employed in raising the debris from the pits. When the bottom of the water-bearing strata was reached, the tubbing, which consisted of metal cylinders cast in complete rings of an internal diameter of 12 ft. and a length of 4 ft. 9 in., was placed in the shaft, the rings of tubbing being connected by bolts. The tubbing was tested by hydraulic apparatus to ½ more pressure than it was expected to be subjected to. The rings of tubbing were let down into the shafts by means of the capstan; the moss-box at the bottom of the tubbing being placed in the pit first. The moss-box consisted of two cylinders, one sliding inside the other, and each having a flange broad enough to form a chamber to hold a quantity of ordinary moss. When the moss-box reached the bed which was prepared for it at the bottom of the pit, the weight of the superincumbent tubbing pressed upon the moss, and formed a water-tight barrier. The tubbing being thus fixed, the annular space between it and the sides of the shaft was filled with cement, thus insuring the solidity of the tubbing; after this was finished the standing water in the shaft was drawn out, and the joint below the moss-box was

made permanently safe by the fixing of several rings of tubbing resting on 2 strong wedging cribs.

The comparative cost of sinking by the processes referred to was shown by two tables, one of which exhibited the complete cost of sinking, and the time occupied by the ordinary system, at 18 different collieries, whilst the other gave the same information for 10 collieries put down by M. Chaudron's process. The results showed that, whilst, with the system of sinking by the aid of pumping machinery, the average cost per foot had amounted to £114.7, and the rate of sinking to 8.9 ft. per month, with the Chaudron process the average cost of all the pits was equal to £22.9 per foot, and the speed of sinking to 15.8 ft. per month. This striking result, which was so much in favor of the Chaudron system, evinced the importance which this mode of dealing with water-bearing strata was likely to have.

It was remarked that, where a large quantity of water occurred in shallow sinkings, tubbing would be of no avail, and the economy of boring by the "Niveau plein" system would probably be considerable. On the other hand, where the strata were hard, and where the feeders of water were so well separated by beds of rock as to allow them to be dealt with separately, the ordinary system of sinking might prove as economical as the Chaudron process. The boring of the shaft by the Chaudron process could not be said to be advisable below the water-bearing strata, as with an increased depth the time which could be utilized in boring would become less, and further, the small particles into which the rock was broken by the tool hindered the sinking, so that it progressed more slowly than when the shafts were sunk by the ordinary mode.

March 12th.—The paper read was "On the Soonkesala Canal of the Madras Irrigation and Canal Company," by Mr. J. H. Latham, M. Inst. C.E.

The object of this communication was to give a general description of the canal between Soonkesala and Cuddapah, recently constructed for irrigation and navigation by the Madras Irrigation and Canal Company; and to direct particular attention to the mode of safely constructing high banks for canals or tanks, illustrated by that work.

KING'S COLLEGE ENGINEERING SOCIETY.—At a general meeting of this Society, held February 23d, Mr. Hunter, President, in the chair, Mr. Baynes read a paper on "Submarine Works." The author began by describing the different methods for laying sea foundations, viz., by piles, either in double or single rows; by caissons; and by rubble masonry of huge blocks, or by masonry laid by divers or diving bells, describing the different machines used. In conclusion, he gave an account of the submarine boats that had at different times been invented, showing the causes which prevented their use, the principle one being the want of view.

IRON AND STEEL NOTES.

HOMOGENEOUS CAST-STEEL AT ONE OPERATION.—The novelty which constitutes the invention of Mr. L. Viger, of Montreal, Canada, is the use of the admixture in predetermined and definite proportions of pulverized plumbago, anthracite, charcoal, bituminous coal, coked or in the natural state, compressed or not, with pulverized iron ores, oxides, or carbonate of iron, iron-sand, or wrought-iron, iron scraps, shavings, clips, and sponge, and metallic iron of any description, in a crucible or reverberatory furnace, or re-heating or puddling or air-furnace, or with what is known as a Siemens furnace, or in any other furnace heated by gas, to make cast-steel of any desired quality in one operation. The mixture, if used in a furnace, to be covered or not with a flux of glass or blast-furnace cinders, or with glass-making materials, slabs of soapstone, tiles, bricks, or any other covering, and if the ore or carbon used contains earthy matter, the slag scoriæ which they will furnish may render other coverings unnecessary. In a furnace heated by gas if a neutral flame, neither oxidizing nor carburizing, can be produced, no covering is required. The above admixture to be used in the following proportions—from two-tenths of 1 per cent. to 30 per cent., and even 40 per cent. of said carbon in weight of the ore used, or of the oxide of iron, or carbonate of iron, according to the purity of the oxide or carbonate of iron, and of the carbon used, and according to the quality of the cast-steel to be produced. The above admixture, either loose or compressed, with a coating of plumbago or other carbonaceous matters, and covered or not with the above-mentioned coverings. To the admixture may be added a small quantity of oxide of manganese, lime and fluate of lime, in equal proportions, wood, tar, or common glue may be added in compressing. To insure uniformity, the ores and carbons used should be free from all impurities, finely pulverized, thoroughly and uniformly mixed, and when weighed must be dry, and sufficient carbon added to insure the carburizing of the ores to the required degree.

AMERICAN PIG METAL FOR BESSEMER STEEL.—Prof. A. A. Fesquet contributes the following article upon this subject to the "United States Railroad and Mining Register:"

"If we examine the history of the Bessemer process in America, we will see that it has passed, and is still passing, through the same stages and difficulties it had to encounter in England.

"In that country, the first attempts made with ordinary kinds of pig-iron, resulted in irregular working, and in a steel of inferior quality. The first successful results were obtained from imported Swedish metal, high in price, and the supply of which was insufficient. Later, when the requisites of a suitable pig-iron were better known, when the various British metals were analyzed and tried, and especially after the great development of the iron manufacture in the Cumberland district, then only was the success of the Bessemer process entirely established in England.

"We might have profited by the experience of the English ironmasters, and availed ourselves of the knowledge they had gained as to the metals to be used; but it would appear that the American steel-maker considered that it was the *process*, the "Bessemer process," which was all that was essential for successful results. Therefore, Bessemer steel works have been erected in various localities of this country, at great cost and perfect in construction, but, to say the least, with indifferent success, for the reason, as we believe, that the proper metal has not been employed.

"The real success of the Bessemer process in this country will date from the time when the purest kinds of American ores shall be smelted in blast furnaces, working for Bessemer pig only. Then will the steel manufacturer be certain of a sufficient supply constant in quality, and the treatment of which will not bewilder the operatives.

"A personal examination of many samples of iron ores from North Carolina, and the analyses recently made by Dr. Genth of similar ores from the same State, convince us that they are singularly well adapted to the manufacture of Bessemer and tool steel, and also of Swedish iron. We base this belief upon facts derived from the composition of the best known qualities of pig metal used abroad for the Bessemer process, and from that of the ores employed.

"The following analyses are of Bessemer pig metal, made at:

I. Cleator (Cumberland district, red hematite).

II. Harrington (Cumberland district, red hematite).

III. Workington (Cumberland district, red hematite).

IV. From English hematite mixed with Titanium ores from Norway. This pig is said to answer well for the Bessemer process.

V. Askam, Furness Iron and Steel Company (red hematite).

VI. Fagersta (Sweden—Magnetite).

VII. Neuberg (Styria—Spathic ores).

	I.	II.	III.	IV.
Metallic iron	93 552	93 100	92.859	93.47
Graphitic carbon.	3 082	2.952	2.997	3.31
Combined Carbon	1.265	1.235	1.134	
Silicum	1.389	2 286	2 706	1.86
Sulphur	0.068	0.075	0.068	0.071
Phosphorus.....	0.027	0.055	0.028	0.076
Manganese	0.216	.288	0.140	0.50
Titanium..... ...	6	6	0.007	1.15
	99.605	99.997	99.930	100.437

	V.	VI.	VII.
Metallic iron	93 191	.....	90 507
Graphitic carbon	3.928	3.527	3 180
Combined carbon.....	0 109	1 012	0 750
Silicum	2.640	0.854	1 960
Sulphur.....	0.004	0 010	0.018
Phosphorus.....	0.014	0 051	0.040
Manganese	0.093	1.919	3.460
Copper.....	.....	.....	0 085
	99.979	.....	100.000

"All these samples show that the proportion of graphite carbon largely predominates over that of the combined. Direct experiment in the converter has proved the necessity of such an excess of graphite, since pig metal with a large percentage of combined carbon, and good for puddling, did not give satisfactory results in the Bessemer process. As the per cent. and nature of the carbon in pig iron are especially due to the ratio between the fuel and the burden, they depend more on the mode of working the stack than on the nature of the ores, although some persons assert that the metal from silicious ores is more readily permeated with graphite.

"The proportion of silicium is also uncommonly large, and the knowledge of its effects is of comparatively recent date. Like carbon, it acts as fuel for supporting the heat during the blast; it makes the charge work hot. This is its only advantage, because, although the greater part of the silicium is oxidized at the begining of the blast, and even before the carbon, there still remains a small proportion of it, sufficient to render the metal hot short, and requiring the addition of spiegeleisen to be entirely eliminated.

This red-shortness of metals made from silicious materials, and free from sulphur, has already been ascertained several times. Cumberland pig, puddled alone, gives a red-short iron. Cast steel, rich in silicium, is also red-short. We have ourselves noticed similar results, in connection with the presence of a slight proportion of iron oxide, in experiments made with the Martin process, when all red-shortness disappeared after the addition of spiegeleisen, or Franklinite, which would not be the case should the metal be sulphurous. Moreover, Mr. Caron has already observed that, under the influence of a high and oxidizing heat, manganese aids in the transformation of the silicium into silica.

"The effect of spiegleisen is thus manifold. It restitutes to the decarburized metal the desired proportion of carbon; it cleanses it from oxides and thick slags, its manganese taking the place of iron and forming very fluid slags; and, lastly, it removes all the silicium.

"We need not dwell on the pernicious effects of sulphur and phosphorus; they are too well known. Although these substances are found in all of the samples of Bessemer pig metal indicated, and which are of superior quality, their proportion is so small as to amount practically to nothing. Nevertheless, in ores, sulphur is the less objectionable of the two, since, by careful roasting, it may be nearly entirely removed. In Styria, for instance, 4 per cent. of sulphur in the ores is not considered a bar to their employment.

"Manganese, found in variable proportions in the above named pig metals, is a valuable substance, so much so that spiegeleisen may be dispensed with in the treatment of certain pigs rich in manganese.

"To sum up, we may say that, provided the ores are practically free from phosphorus and sulphur, they are suitable for the manufacture of Bessemer pig; and such is the case with the iron ores of North Carolina referred to, which have the additional important merit of containing a notable percentage of manganese and chromium.

"Red hematites are not, as some people state, the best or only ores for the manufacture of Bessemer pig. It should be remembered that the Bessemer process was first rendered a success in England through the use of Swedish pig made from magnetic ores, and that, at the present time, Sweden is still ahead in regard to the quality of its Bessemer steel. The development of the red hematites

of Cumberland made a cheaper metal—good, indeed, but not better than the Swedish one. The great advantage for England was a home product, of which a large supply could be had at a reasonable price.

"Next after Sweden, in the quality of its products, comes Styria, with its metal from spathic ores. At the same time, we do not wish to convey the idea that *pure* red hematites are inferior to *pure* magnetic or spathic ores. We believe that the superiority of the Swedish or Styrian products is due to a greater care in the roasting process, and to the purity of their fluxes and fuel (charcoal).

"The ores which we have examined as coming from North Carolina are of the primary kinds—that is, principally magnetites and compacted hematites. In places, as in Sweden, they become mixed, the hematites running into magnetites, and conversely.

"The magnetites are either pure or mixed with titanic acid. All the pure magnetites which we have examined are sufficiently rich for the blast furnace, yielding from 40 to 50 per cent. and over of metal. Should it be desirable to enrich them further for the bloomery fires, this may easily be done by means of magnetic machines, for, unlike many of the magnetic ores of Northern New York, the grains of quartz do not strongly adhere to the magnetic portions, and are completely separated by the crushing process. Many kinds of the titaniferous magnetites may be treated in the same manner, and titanic acid left behind. Some of the pure magnetites examined contain manganese, and they are remarkably free from sulphur and phosphorus.

"The titaniferous magnetites are especially worthy of consideration. They are said to exist in great abundance, and are free from sulphur and phosphorus; they produce the most excellent quality of tool steel, and average 55 per cent. of metal, from 10 to 12 per cent. of titanic acid and from 1 to 2 per cent. of manganese and chromium. They are superior to all other kinds of ores for the lining of puddling furnaces. They will, mixed in proper proportion with other ores, and with suitable fluxes, greatly increase the quality and toughness of the metal. The fluxes to be preferred for such ores are the multiple silicates holding metallic bases, and these are abundantly found in the immediate vicinity. We have personally examined metals cast directly from titaniferous ores, without admixture of other ores; and there are analyses of slags from Swedish blast furnaces holding about 10 per cent of titanic acid. Several of the samples of Bessemer pig above given show the presence of titanium; one especially, where the proportion is quite large. The per cent. of titanic acid in the ore must have been considerable, since it is well known that the greater part of that substance goes into the slags.

"The compact red hematites of this part of North Carolina are also remarkable for their purity, abundance, and considerable yield of metal. They are silicious, like the Cumberland hematites. This is an advantage for the manufacture of Bessemer pig, which requires silicium; but should these ores be desired for other purposes, the excess of free silica may be neutralized with aluminous magnetites, also found in the vicinity.

"Spathic ores, crystallized or compact, have been found, but their development has not yet been sufficient to determine their extent.

"The fluxes necessary to the smelting of all these varieties of ores are said to be abundant. They are granular limestone, clays, ochres, garnets, and the gangues of the ores themselves.

"Charcoal is abundant in the immediate vicinity of the ores, and will be so for many years to come. Moreover, the important coal fields of the Dan and Deep rivers are there to supply any deficiency in vegetable fuel.

"On pig metal manufactured from iron ores such as we have described, in whatever locality they may be found, depends, in our belief, the profitable success of the steel and of the first class bar iron manufacture in this country."

RAILWAY NOTES.

RAILWAYS IN TURKEY.—The Government is making strenuous exertions to extend the lines of road. Important works are in operation to unite Constantinople with the capitals in Western Europe. There is also a very important project to connect Scutari with a network of roads in Asiatic Turkey, ultimately destined to unite India and Europe. The first section from Scutari to Ismid (the ancient Nicomedia), 90 kilometres long, will be open for traffic in September next. The extension to Eski-Schehir, 200 kilometres further, is surveyed, and a provision made for a capital for its construction.

PERFORMANCE OF A LOCOMOTIVE.—The following extract from a latter, detailing the performance of one of Baird & Co.'s locomotives, will be read with interest.

"Engine 422 has been taken into the shop for repairs, and as her performance has been an extraordinary one, you will, I am sure, desire to know some of the particulars of her career thus far.

"She was placed on the road on the 17th day of October, 1867, and ran until the 14th day of May, 1871. During the whole of this time she hauled fast and heavy passenger trains over Middle Division, and made the wonderful run of 153,280 miles, losing only 3 trips, which was during November, 1869, to have 6 new flues put in and to clean the mud out of the waist of the boiler. She also lost 6 round trips in May, 1870, getting a larger tank, to enable her to make the run from Altoona to Harrisburg (132 miles) without a stop.

"This, however, was no fault of the engine and should not be counted against her.

"As an offset against the nine trips lost, she made 11½ extra trips between Altoona and Harrisburg.

"The total cost for repairs up to the time she was laid off amounted to $3,727.06, or 2.44 cents per mile. Our book account makes these amounts somewhat greater, but I have deducted all items not actually running repairs, such as the new tender, cost of applying air brakes, etc., which, although under our system of accounts are necessarily charged to repairs, actually do not belong there.

"When Engine 422 was taken into the shop, she was reported as run down in the working parts, but uniformly so; all the bearing surfaces being smooth and good and her general condition being better than is usual with engines taken in for repairs. The cost of placing her in thorough repair is estimated at $1,262.73."

ENGINEERING STRCTURES.

THE DRAW-SPAN OF THE DAVENPORT BRIDGE.—The great draw-span of the new bridge over the Mississippi at this point was circled or turned for the first time at noon yesterday. It is the longest drawbridge on the Mississippi, and the heaviest in America, if not in the world. Yet ten men pulled it round quite easily, with the aid of a single pulley, heavily loaded with lumber from end to end though it was. Such of our readers as have not had an opportunity of examining this great work will be interested in a description conveying an idea of its tremendous strength. The draw-span as it stands is a Whipple truss inverted; that is to say that its top chord is in tension and its bottom chord is in compression, which is exactly the reverse of the style of the fixed spans of the bridge. The whole strain of the draw-truss is carried right to the centre from the ends, while in the fixed spans the strain is transmitted from the bottom of the posts up to the tie-bars to the ends, throwing the top chord into compression. The draw is, in exact figures, 366 ft. and one quarter of an inch in length and the posts 46 in number. The posts, to be particular, are connected by top and bottom chords, top and bottom struts and diagonal lateral bracing. The weight of the iron in the span, exclusive of the turn-table, is 871,784 lbs., or about 426 tons.

The turn-table is a new invention of C. Shaler Smith, President and Chief Engineer of the Baltimore Bridge Company, and this is its first application. It differs materially from any other work of the kind. In describing it, we will commence with the bed-circle itself, resting on the centre or pivot pier. The circle is 32 ft. in diameter, and is composed of six segments, each 8 in. deep by 12 in. wide, and weighing 6 tons—36 tons for the circle The top surface is bevelled, the inner side of the surface being highest. On this bed-circle are mounted 36 heavy cast-iron wheels, 2 ft. 6 in. in diameter, with a 12-in. face; through the centre of each wheel is placed an adjustable tie-rod, which runs to the centre-pin as a radial bar; the wheels are also spaced at correct relative distances by an inner and outer set of distance-plates, which, with the radial rods, regulate the distance and travel of each wheel in the circle. The wheels are cast, turned and faced up to a bevel exactly corresponding to the bevel surface of the lower bed castings, but placed with their greatest diameter on the outer side of the bed-circle, thus giving to each wheel an enlarged circumference to travel on the increased circumference of the outer side of the bed-circle over the circumference of the inner side. Thus each wheel, from its formation and the formation of the bed on which it moves, naturally tends to travel in a segment of the circle, and by avoiding the tendency which square-faced wheels have to travel in right lines or on a tangent, escapes any severe tension on the centre of the radial rods, consequently avoids the severe friction which would otherwise be inevitable. On the wheels above described is mounted a rotary table, formed in six segments, averaging 5 tons each, and 5 ft. in depth, which are accurately fitted together at their joints, and secured by heavy keys and bolts, forming a circle as correct and solid as though cast in one immense piece.

On the exact radial centre of the masonry is mounted a huge centre-pin bearing, 2 ft. 8 in. high, with a base 4 ft. in diameter. Across the top of this and bolted into the inner surface of the rotary table are two cast struts or cross beams of immense weight (averaging 6 tons each) and great strength, which render the office of transmitting to the centre-pin bearing and the rotary bed their correct relative proportions of the pondrous weight brought upon this strut by the main centre post of the superstructure, the main posts being mounted upon heavy cast-iron shoes seated upon the main girder. Besides this girder or strut, crossing the centre-pin and taking eold of the rotary table, there also radiate from the centre-pin bearing and from the centre of the main girder numerous struts and tension-rods of wrought iron, which serve the purpose of keeping at all times the rotary bed in correct tram or perfect circle from the fixed centre. The exact weight of this turn-table, exclusive of the power spoken of below, is 205,416 lbs.—almost 103 tons.

The rotary power which is to turn this great drawbridge has rather a novel method of application, as any one can see.

Immediately over the centre portal arch will be placed a reservoir of wrought iron, to contain about 3 barrels of pure glycerine, which will flow down through tubing into 4 hydraulic pumps which will be worked by a steam engine placed on a level with the railroad fleck and will be forced by the pumps into two huge "rams" or "jacks" placed on each side of the span at the centre posts. From the plunger of each "jack" will be led a wire cable 1½ in. in diameter, so arranged that as the plunger of the "ram" on one side of the span is ascending and shortening the cable lead on his side, the other "ram" will be descending and passing out or lengthening his cable—and as one "jack" shortens the cable attached to it, he draws himself, and consequently the side of the span, toward the point where the other end of his cable is permanently made fast in the solid masonry of the pivot pier; and at the same time the other "ram" passing out or giving line on his side of the truss permits the span to rotate in accordance with the pull of the other "ram," and is prepared at a moment's notice to act as a brake, and entirely check the span, or to cause it to turn in an opposite direction.

Either "jack" can pull, and either one can hold back, as the supply and discharge of pipes at the top and bottom of each "jack" are arranged with a view to make them act as reciprocators.

No fluid is lost by working, further than by leakages at joints and valves; while the hydraulic pumps are intended to be worked by steam power, they are also arranged for being worked by "hand power," in case of necessity.

The rotary power of this draw has been designed with especial view to perfect simplicity and durability, and avoidance of use of gearing of any kind; nor is there anything new or scientific in the plans adopted.—*Exchange.*

DETROIT RIVER TUNNEL.—Work on the (Detroit side) shaft began Dec. 1, 1871, and was finished Jan. 31, of this year, when the bed rock was reached, 108 ft. below the surface of the river: depth of masonry, 114 ft; upper 89 ft., is 15 ft. diameter and 16 in. thick, the remaining 25 ft. has diameter of 9 ft. with 12 in. walls. The drainage tunnel starts from bottom of shaft, 8 ft. above rock, leaving well below for water that may

come into shaft. Drainage drift excavated under river to point 130 ft. from shaft, a daily average of 5 ft., through very hard clay, having layer of boulders, from a few ft. to a cubic yard in size, half way up in drift. Latterly these are smaller and less frequent, and it is thought will either run out or dip below bottom of drift. Portions of tunnel have stood well a week unsupported, but tunnel is carefully lined with masonry within a day or two —an 8 in. circle of the hardest brick, every one subjected to rigid inspection and laid in hydraulic cement. The drift rises from the shaft to centre of river on a grade of 1 ft. in 1,000. The line was located above ground by triangulation, and transferred below by means of plumb-bob in water, suspended down shaft by fine silk cords. As the distance between them was so short that an error of 1-32 in. would throw the work out 6 or 8 in. at middle of river, a drift has been run back from the shaft 50 ft., and an iron tube is being sunk to its end, in which a plumb-line may be suspended and a longer range obtained. The drainage drift is not straight across the river, but has two short curves in it. Work has been commenced on shaft at Windsor, and tunnel will be excavated from it to meet that being worked from Detroit side. It is anticipated that the main tunnels will be surrounded their whole length with good solid ground, suitable for construction—firm blue clay. Developments have thus far been quite as favorable as was expected when the plans and estimates were prepared, and show nothing to discourage a reasonable belief that the work will be carried to a successful issue.

HOOSAC TUNNEL.—This great work made material progress in 1871. The distance from eastern portal to central shaft is 12,837 ft., and of this 5,283 ft., or one yard over a mile of heading, had been penetrated by State engineers, first April, 1869, when the present contractors took charge. This left 7,554 ft., and of this but 2,501 ft. of heading remained on first Feb., 1872. On the west side more and greater difficulties have been encountered, and there remain 4,644 ft. It is expected that the tunnel will be done and cars pass through it in autumn of 1874.

Railway connection with E. portal will be soon completed; and two miles of new R. will connect W. portal with North Adams racks.

BOOK NOTICES.

HAND-BOOK TO SOUTH AFRICA. S. W. Silver & Co., London.

This hand-book contains much useful advice and information for those about to visit the diamond and gold fields of South Africa, and it is one of a series that the Messrs. Silver have always been ready to publish for the assistance and guide of emigrants and *voyageurs*.

An excellent preface has been written by Mr. H. W. Bates, F. R. G S., giving in a concise manner the principal geographic peculiarities and formations of the country. The routes to the diamond fields are pointed out, and the best means to be adopted in furtherance of such a route as may be selected. The general advice appears to agree with that given by all who have tried the diamond fields —that it is useless to attempt to go there and prosecute diamond hunting without sufficient capital, provision being made for the return journey in the event of disappointment.

The hand-book contains the following ready rule for the determination of heights, by the use of an aneroid barometer: If you require only to ascertain the difference of height of two places, A to B, the following is all that is necessary. Read the aneroid at A, say 30.15, take it to B, read it there, say 29.08, take it back to A, read it again, say 30.19. Then take the mean of the readings at A, and find the difference between that and the reading at B. Multiply the difference in hundredths by 9, and the result will be the difference of altitude in feet, thus:

$$\frac{30.15+30.19}{2} = 30.17-29.08 = 1.09,$$

then 109×9—981, height in feet.

ESTIMATES AND DIAGRAMS OF RAILWAY BRIDGES. By J. W. GROVER, C. E. Second Edition, enlarged. London: Spon. For sale by Van Nostrand.

These estimates and diagrams include railway bridges for turnpike, public and occupation roads, in the embankments of double and single lines and cuttings of double lines, with form for calculating quantities in skew structures, etc.; also culverts of various dimensions. The author, in this edition of a strictly professional and reliable work, has added some good examples of station works, as actually executed, with their costs, which will be found useful.

TABLES FOR PLATE-LAYERS. By WILLIAM DONALDSON, M.A., A.I.C.E. London: Spon. For sale by Van Nostrand.

These tables are compiled from the formulæ of the work on "Switches and Crossings," by the same author, and lately noticed in our columns. The tables, relating to various gauges, single and mixed, are preceded by an explanation. The volume must be a useful one to plate-layers; it is cased in stout boards, and can be opened without breaking the back of the book, which is what cannot be said of every volume nowadays.

PRACTICAL GEOMETRY FOR THE ARCHITECT, ENGINEER, SURVEYOR, AND MECHANIC. By E. WYNDHAM TARN, M.A., Architect. London: Lockwood & Co., 1871.

This is a work of remarkable merit in its class, and will be found of real use by those to whom it is addressed. The rules laid down are simplified, and they are fully illustrated by 164 wood engravings. We should have been glad to find a separate chapter on the theory of domes, concerning which Mr. Tarn has elsewhere shown himself well informed.

TEXT-BOOK OF GEOMETRY. Part I. By T. S. ALDIS, M. A., Professor of Mathematics, Newcastle College of Science. London: Bell and Daldy, 1871.

This is an attempt to reconstruct the contents of the first three books of Euclid. The author, to make his treatment of the subject valuable as an exercise in mental rather than mechanical reasoning, has shortened the proofs given by omitting minor points, which are left as exercises for the student. The work being theoretical, hypothetical constructions are throughout employed, and prob-

lems are given as exercises, the proofs of the constructions being in most cases left to the student. Professor Aldis divides his book—which he proposes to continue—into three sections. The first of these treats of Angles, Parallels, Triangles, and Theorems of Equality and Inequality. The second, of Parallelograms and Equivalent Figures; and the third, of Circles and Polygons and their various properties. The book is worthy of the notice of Geometricians as being one of the most respectable attempts among many barren failures to supersede Euclid as a text-book. The development of the author's plan must have cost him much time and reflection, and the result is not at all discreditable to his distinguished position as a mathematician. We have said enough to indicate the character of Mr. Aldis's present effort; we shall have to wait till we see its extension before we can be able to give an opinion as to the character of his scheme as an educational method.

THE YEAR BOOK OF FACTS, 1872. By JOHN TIMBS. London: Lockwood & Co. For sale by Van Nostrand.

This work, which has been before the public so many years, is too well known to require any eulogium at our hands. The volume for 1871 is just published, and contains the usual notices pertaining to science and art. We would suggest in another volume a little more variety in the quotations from newspapers.

PLAIN DIRECTIONS FOR THE CONSTRUCTION AND ERECTION OF LIGHTNING-RODS. By JOHN PHIN, C. E. New York: The Handicraft Publication Company. For sale by Van Nostrand.

This little hand-book will prove of signal service, in extending a knowledge of the laws of Atmospheric Electricity, and so serve to check the frauds perpetrated in the rural districts by charlatans who engage in the business of erecting lightning-rods.

The author writes much in the interest of practical science, always concisely and correctly.

POCKET-BOOK OF MECHANICS AND ENGINEERING. By JOHN W. NYSTROM, C. E. Philadelphia: J. B. Lippincott & Co. For Sale by Van Nostrand.

This is a revision and an enlargement of a well-known book, which in this issue appears as the eleventh edition. Nothing seems to be wanting which an engineer expects to find in his pocket-book. The tables are more than ordinarily complete, both Natural and Logarithmic. Trigonometric functions are given for single minutes and to five places of decimals. Not the least valuable portion is that relating to the steam engine, which extends over 32 pages. The volume contains 510 pages.

AN ELEMENTARY AND PRACTICAL TREATISE ON BRIDGE BUILDING. By S. WHIPPLE, C. E. New York; D. Van Nostrand.

A new work from the pioneer in scientific bridge building in this country is an event worth noting.

Only in the first chapters does the present work bear the familiar features of the older editions; the book is greatly enlarged by the addition of much new and altogether valuable matter.

Discussions of the principles of construction of the Fink, Bollman, Post, and Warren Trusses, are given in terms that cannot be misunderstood even by readers who habitually shun mathematical formulæ.

No work within our knowledge is so thoroughly practical in the matter of details of selection and distribution of material, as well as in uniting of the different members of a truss. That clearness of elucidation which recommended the old work so strongly to young engineers, is equally a characteristic of the present edition. It is printed in excellent type and contains an abundance of illustrations interspersed with the text.

MISCELLANEOUS.

THE durability of the framed timbers of buildings is very considerable. The trusses of the old part of the roof of the Basilica of St. Paul, at Rome, were framed in 816, and were sound and good in 1814, a space of nearly a thousand years. These trusses are of fir. The timber work of the external domes of the Church of St. Mark, at Venice, is more than 840 years old, and is still in a good state. And Alberti observed the gates of cypress to the Church of St. Peter, at Rome, to be whole and sound after being up nearly 600 years. The inner roof of the Chapel of St. Nicholas, King's Lynn, Norfolk, is of oak, and was constructed about 500 years ago. Daviller states, as an instance of the durability of fir, that the large dormitory of the Jacobins' Convent, at Paris, had been executed in fir, and lasted 400 years.

A CRUCIBLE for melting metal has been invented, which consists in providing the ordinary crucible of plumbago or other substance with a flue or passage from the bottom to the top, for allowing the heat to act upon the centre of the mass of metal contained in the crucible more directly than it otherwise can. This passage is surrounded by a shell or tube of the same material of which the crucible is made. The inventor also grooves, or indents, or constructs the sides of the crucible, both inside and out, so as to form projections, to interlock with the paste, or clay, or other substance with which the crucible is coated, to cause the coatings to be retained much longer than they now are, thereby preserving the crucible much longer, and reducing the cost of melting steel or other metals.

THE SUEZ CANAL.—The Suez Canal Company held a special general meeting on the 12th inst., to receive the report of the council, and to consider some new propositions, or rather to be informed of fresh decisions of the council.

The report respecting the working of the canal is very satisfactory. The number of ships passing through the canal has increased from 765 in the whole year of 1871, to 200 in the first two months of 1872; and the total receipts from a little over five millions of francs for the former, to two-and-a-half millions for the latter period. The cost of working has at the same time been reduced, and the promised result is that there will be at the end of the present year a clear balance of, at least, £89,200 on the twelve months' working.

The canal is now reported to have an average depth from the extremity of the jetties of Port Said to the roads of Suez, of no less than 8.30 me-

tres, in proof of which the transit of the Nebraska is mentioned, a ship of 2,983 tons, and drawing 7.21 metres of water. The reports speak also of the passage without difficulty of the great transports of the English Government, of 3,000 tons burthen, and one of which had 1,421 soldiers on board; of the Peiho of the Massageries Company, making the transit in less than 14, and the Persia packet in less than 13 hours.

The engineers report, after an experience of two years, that the maintenance of the canal proper will entail an anual expenditure of £33,000 per 500,000 cubic metres.

Another fact on which the company is to be congratulated, is that the council has only issued twelve millions of the twenty million loan voted in July last.

AT the Southern Railway terminus at Vienna, the water supplying the locomotives is rendered soft by the use of lime water, and is subsequently passed through filters of peculiar construction to remove the precipitate. In the table below are given (1) the solid contents of 10,000 parts of the water before treatment; (2) that of the same amount after treatment; (3) percentage composition of the boiler scale from the hard water; (4) that of the mud for the softened water; and (5) the composition of the contents of the filters.

	1.	2.	3.	4.	5.
Chloride of sodium	0.8029	0.8237	.....	....	....
Chloride of magnesium	0.2986	0.2892	....	....	....
Sulphate of lime ..	1.9398	1 6796	2.29	76.60	7.92
Carbonate of lime.	1 8830	0.0292	73.87	1.41	69.71
Carbonate of magnesia	1 4729	0 0178	19 40	1.57	10.96
Oxides of iron and alumina	.....	.....	3.07	1 52	3.46
Silica.....	0 0715	0 0580	.....	....	....
Sand	.....	....	0 83	0 65	0 63
Organic matter. .	1.9853	1.4370	0 93	18.23	5 57
	8.4540	4.3345	100 39	99.98	98.25

It is seen that the carbonates of lime and magnesia are almost entirely, and the sulphate of lime (gypsum) is partially, removed by the process, while the amounts of organic matter and silica are considerably diminished. If the quantity of sulphate of lime be more considerable, the water may be freed from it after the lime treatment by the addition of soda solution, which converts the remaining gypsum, the chloride of magnesium, and chloride of calcium, if present, into an insoluble form that can be separated by means of the filter.

IT seems that the proposition to tunnel under the English Channel is not to end in talk, after all. The company registered in London on the 15th ult., is about to excavate a trial shaft and driftway on the English side, and, if the scheme is found practicable, push the work on until it reaches the shores of France. A tunnel 22 miles long, and under water at that, is a formidable undertaking, but it is evident the company mean business, since the experimental section to be built, which will have no value whatever except as a part of the finished work, will cost £2,000,000. So much capital is not likely to be wasted in the hands of English engineers, and when the work is once fairly undertaken, we shall expect to see it completed.

GRANITE WORKS OF THE ANCIENTS.—We quote the following from an exceedingly interesting account of the colossal granite structures of ancient Egypt, India, and South America, which appears in the current number of the "People's Magazine:" "The art of carving in granite has never been carried to higher perfection than on the continent of India. At Chillambaram, also in the Carnatic, and on the Coromandel coast, is a congeries of temples, representing the sacred Mount of Meru. Here are 7 lofty walls, one within the other, round the central quadrangle, and as many pyramidal gateways in the midst of each side, which form the limbs of a vast cross; consisting altogether of 28 pyramids. There are consequently 14 in a line, which extend more than a mile in one continuous direction! Nor are these the only wonders associated with this metropolis of pyramids. The interior ornaments are in harmony with the whole; from the nave of one of the principal structures there hang, on the tops of 4 buttresses, festoons of chains, in length about 548 ft. Each garland, consisting of 20 links, is made of one piece of granite, 60 ft. long; the links themselves are monstrous rings, 32 in. in circumference, and polished as smooth as glass. Compared with the monolith temples of granite at Mahabalipuram, which is likewise situated on the Coromandel coast, those in Egypt sink into insignificance. The rocks thereabouts are composed of a hard gray granite, containing quartz, mica, and felspar, with a few crystals of hornblende interspersed. Many have been hollowed out by art, and sculptured into temples with spirited bas-reliefs, representing episodes in Hindoo history and mythology, and supported by graceful columns; all carved from the solid rock. Detached masses have been cut into shapes of elephants, tigers, lions, bulls, cats, monkeys, and various nondescript monsters, and colossal statues of gods, one of which, namely, that of Ganesa, being 30 ft. high. The southernmost of the temples is about 40 ft. in height, 27 ft. in breadth, and nearly the same in length; the exterior being covered with elaborate sculptures. The adjoining edifice is about 49 ft. in length, and in breadth 25 ft.; it is rent by natural causes from summit to base. According to the local Brahminical tradition, these wonderful sculptures were executed by 4,000 workmen, who had come from the north, and returned before their completion. From a careful examination, it is evident that almost all the enormous mass of sculpture and carving that adorns this city of monolith temples and colossi, must have been performed without the aid of fire—with the hammer, chisel, lever, and wedge alone; and this is one of the hardest rocks in the world!"

THE Yokohama and Tokio Gas Company is formed for the purpose of lighting the principal city and chief commercial town in Japan with gas. This is another proof of the eagerness with which this people are availing themselves of the applications of science, which have been, until of late years, confined to Europe and America.

CALORIMETER.

Constructed by CAPTAIN JOHN ERICSSON.

Fig. 1.

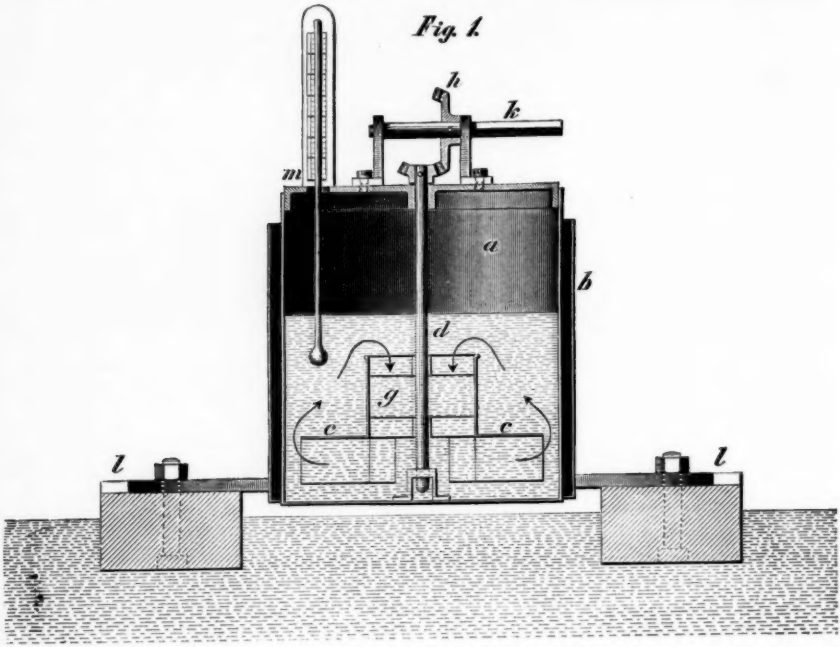
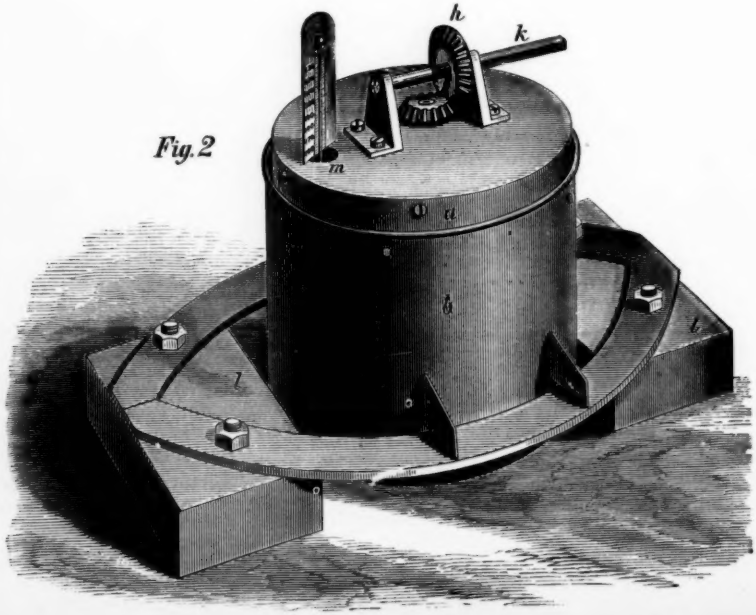



Fig. 2